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HEALTH TECHNOLOGY CASE STUDY 24:

Variations in Hospital Length of Stay: Their Relationship to Health Outcomes

AUGUST 1983

This case study was performed as a part of OTA's Assessment of

Medical Technology and
Costs of the Medicare Program

Prepared under contract for OTA by:
Mark R. Chassin, M.D., M.P.P., M.P.H.

OTA Case Studies are documents containing information on a specific medical technology or area of application that supplements formal OTA assessments. The material is not normally of as immediate policy interest as that in an OTA Report, nor does it present options for Congress to consider.

CONGRESS OF THE UNITED STATES
Office of Technology Assessment
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Preface

Variations in Hospital Length of Stay: Their Relationship to Health Outcomes is Case Study 24 in OTA's Health Technology Case Study Series. It was prepared in response to a request by the Senate Finance Committee, Subcommittee on Health, and is part of OTA's project on *Medical Technology and Costs of the Medicare Program*, requested by the House Committee on Energy and Commerce and its Subcommittee on Health and the Environment and the Senate Finance Committee, Subcommittee on Health. A listing of other case studies in the series is included at the end of this preface.

OTA case studies are designed to fulfill two functions. The primary purpose is to provide OTA with specific information that can be used in forming general conclusions regarding broader policy issues. The first 19 cases in the Health Technology Case Study Series, for example, were conducted in conjunction with OTA's overall project on *The Implications of Cost-Effectiveness Analysis of Medical Technology*. By examining the 19 cases as a group and looking for common problems or strengths in the techniques of cost-effectiveness or cost-benefit analysis, OTA was able to better analyze the potential contribution that those techniques might make to the management of medical technology and health care costs and quality.

The second function of the case studies is to provide useful information on the specific technologies covered. The design and the funding levels of most of the case studies are such that they should be read primarily in the context of the associated overall OTA projects. Nevertheless, in many instances, the case studies do represent extensive reviews of the literature on the efficacy, safety, and costs of the specific technologies and as such can stand on their own as a useful contribution to the field.

Case studies are prepared in some instances because they have been specifically requested by congressional committees and in others because they have been selected through an extensive review process involving OTA staff and consultations with the congressional staffs, advisory panel to the associated overall project, the Health Program Advisory Committee, and other experts in various fields. Selection criteria were developed to ensure that case studies provide the following:

- examples of types of technologies by function (preventive, diagnostic, therapeutic, and rehabilitative);
- examples of types of technologies by physical nature (drugs, devices, and procedures);
- examples of technologies in different stages of development and diffusion (new, emerging, and established);
- examples from different areas of medicine (e.g., general medical practice, pediatrics, radiology, and surgery);
- examples addressing medical problems that are important because of their high frequency or significant impacts (e.g., cost);
- examples of technologies with associated high costs either because of high volume (for low-cost technologies) or high individual costs;
- examples that could provide information material relating to the broader policy and methodological issues being examined in the particular overall project; and
- examples with sufficient scientific literature.

Case studies either are prepared by OTA staff, are commissioned by OTA and performed under contract by experts (generally in academia), or are written by OTA staff on the basis of contractors' papers.

OTA subjects each case study to an extensive review process. Initial drafts of cases are reviewed by OTA staff and by members of the advisory panel to the associated project. For commissioned cases, comments are provided to authors, along with OTA's suggestions for revisions. Subsequent drafts are sent by OTA to numerous experts for review and comment. Each case is seen by at least 30, and sometimes by 80 or more outside reviewers. These reviewers may be from relevant Government agencies, professional societies, consumer and public interest groups, medical practice, and academic medicine. Academicians such as economists, sociologists, decision analysts, biologists, and so forth, as appropriate, also review the cases.

Although cases are not statements of official OTA position, the review process is designed to satisfy OTA of each case study's scientific quality and objectivity. During the various stages of the review and revision process, therefore, OTA encourages, and to the extent possible requires, authors to present balanced information and recognize divergent points of view.

Health Technology Case Study Series[a]

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24	Variation in Length of Hospital Stay: Their Relationship to Health Outcomes; Mark R. Chassin (OTA-HCS-24)

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[b] Original publication numbers appear in parentheses.

[c] The first 17 cases in the series were 17 separately issued cases in *Background Paper #2: Case Studies of Medical Technologies*, prepared in conjunction with OTA's August 1980 report *The Implications of Cost-Effectiveness Analysis of Medical Technology*.

[d] Background Paper #3 to *The Implications of Cost-Effectiveness Analysis of Medical Technology*.

[e] Background Paper #5 to *The Implications of Cost-Effectiveness Analysis of Medical Technology*.

[f] Background Paper #1 to OTA's May 1982 report *Technology and Handicapped People*.

[g] Background Paper #2 to *Technology and Handicapped People*.

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Glossary of Terms

Case mix: The relative frequency of various types of patients, reflecting different needs for hospital resources. There are many ways of measuring case mix, some based on patients' diagnoses or the severity of their illnesses, some on the utilization of services, and some on the characteristics of the hospital or area in which it is located.

Effectiveness: Same as efficacy (see below) except that it refers to average or actual conditions of use.

Efficacy: The probability of benefit to individuals in a defined population from a medical technology applied for a given medical problem under ideal conditions of use.

Length of stay (LOS): The number of days a patient remains in the hospital from admission to discharge.

Medicaid: A Federal program that is administered and operated individually by each participating State government that provides medical benefits to certain low-income persons in need of health and medical care.

Medical technology: The drugs, devices, and medical and surgical procedures used in medical care, and the organizational and supportive systems within which such care is provided.

Medicare: A nationwide, federally administered health insurance program authorized in 1965 to cover the cost of hospitalization, medical care, and some related services for eligible persons over age 65, persons receiving Social Security Disability Insurance payments for 2 years, and persons with end-stage renal disease. Medicare consists of two separate but coordinated programs—hospital insurance (part A) and supplementary medical insurance (part B). Health insurance protection is available to insured persons without regard to income.

Morbidity: A measure of illness, injury, or disability in a defined population. It is usually expressed in general or specific rates of incidence or prevalence. Sometimes used to refer to any episode of disease. See also "mortality (death)."

Mortality (death): A measure of deaths, used to describe the relation of deaths to the population in which they occur. The mortality rate (death rate) expresses the number of deaths in a unit of population within a prescribed time.

p value: In a randomized clinical trial, the probability of concluding that there is a difference between the treatment groups when, in fact, there is none. Also called "Type I error" or "alpha" and commonly called the "level of statistical significance;" analogous to "false positive."

Professional Standards Review Organizations (PSROs): Community-based, physician-directed, nonprofit agencies established under the Social Security Amendments of 1972 to monitor the quality and appropriateness of institutional health care provided to Medicare and Medicaid beneficiaries.

Randomized clinical trial (RCT): An experimental design by which human or animal subjects are randomly assigned either to an experimental group (in which subjects receive the treatment being studied) or to a control group (in which subjects do not receive the treatment being studied). Also referred to as "randomized controlled clinical trial" or "controlled clinical trial."

Reliability: A measure of the consistency of a method in producing results. A reliable test gives the same results when applied more than once under the same conditions. Also called "precision."

Risk: A measure of the probability of an adverse or untoward outcome and the severity of the resultant harm to health of individuals in a defined population and associated with use of a medical technology applied for a given medical problem under specified conditions of use.

Safety: A judgment of the acceptability of risk (see above) in a specified situation.

Validity: A measure of the extent to which an observed situation reflects the "true" situation. *Internal validity* is a measure of the extent to which study results reflect the true relationship of a "risk factor" (e.g., treatment or technology) to the outcome of interest in study subjects. *External validity* is a measure of the extent to which study results can be generalized to the population that is represented by individuals in the study, assuming that the characteristics of that population are accurately specified.

Statistical significance: See "p value."

Statistical power: In a randomized clinical trial, the probability of detecting a difference between the treatment groups when one does exist. Failure to detect an effect is called "Type II error" or "beta;" analogous to "false negative."

Synthesis: The integration of findings from different studies and the development of generalizations based on their results.

Type I error: See "p value."

Type II error: See "statistical power."

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Glossary of Acronyms

CHF — congestive heart failure
DRG — Diagnosis Related Groups
IOM — Institute of Medicine
LOS — length of stay
MI — myocardial infarction
NCHS — National Center for Health Statistics
PAS — Professional Activities Survey
PSRO — Professional Standards Review Organization

BCPSRO — Baltimore City PSRO
BPSRO — Baltimore area PSRO
CMPSRO — Central Massachusetts PSRO
CM-PSRO — Central Maryland PSRO
MFMC — Multonomah (Portland, Oreg.) Foundation for Medical Care
UPSRO — Utah PSRO
RCT — randomized clinical trial

1.
Introduction and Executive Summary

1. Introduction and Executive Summary

Hospital length of stay (LOS) varies markedly and persistently across geographic areas in the United States. This phenomenon is the most striking of all the variations in health service use that have been observed. For example, since 1965 the Northeast census region has maintained an average LOS that is about 2½ days longer than that of the West (130). In 1979, Medicare average LOS varied from a high of 13.1 days in one New Jersey Professional Standards Review Organization (PSRO) area to a low of 6.0 in a California PSRO area (74).

Policymakers have been tempted to conclude that it is the longer lengths of stay that are inappropriate and should be curtailed. The potential economic significance of these variations is large, and this fact has not escaped the attention of those charged with the administration of public health care programs such as Medicare and Medicaid. If all four census regions had experienced the West's age-specific lengths of stay in their own hospitalized populations in 1980, patients in regions outside the West would have spent some 44.3 million fewer days in the hospital, a saving of 16 percent (129). Patients 65 years of age and over would have spent 20.6 million fewer days, a reduction of 20 percent. Of the remaining three census regions, the Northeast accounted for 42 percent of the "excess" days, the Northcentral for 36 percent, and the South for 22 percent.

BACKGROUND

As the search for solutions to rising health care costs has intensified over the past few years in both the public and private sectors, the implications of geographic variations in LOS have assumed greater importance. Concern over the costs of the Medicare program has led policymakers and researchers to examine the underlying reasons for the variations with the hope of cost containment. Yet before determining whether variation in LOS can provide the basis for policies to contain Medicare costs, several key questions must be addressed. First, are the LOS differences simply the result of differences in demographic characteristics or severity of illnesses among the different populations? Second, if the populations are comparable, what do physicians do differently that leads to different lengths of stay? Finally, and perhaps most important, do the differences in LOS lead to differences in patient health outcomes?

As background to its deliberations over Medicare costs, the Senate Finance Committee's Subcommittee on Health requested that OTA examine the evidence on variations in lengths of hospital stay and their relation to health outcomes. This case study presents the results of this examination. It was prepared as part of OTA's project on *Medical Technology and Costs of the Medicare Program.* The entire project is being conducted in response to requests by the House Committee on Energy and Commerce and its Subcommittee on Health, and the Subcommittee on Health of the Senate Committee on Finance.

On April 20, 1983, Public Law 98-21 provided for extensive changes in Medicare reimbursement policies for hospital-based care. Under the statute, whose provisions will be phased in over 3 years, hospitals will receive a flat fee per patient, set prospectively, on the basis of patient diagnosis in one or more of 467 Diagnosis Related Groups (DRGs).* Because the payment for any DRG will not increase as an individual patient's LOS increases, answers to the questions raised above are even more critical.

*A recent technical memorandum by OTA entitled *Diagnosis Related Groups (DRGs) and the Medicare Program: Implications for Medical Technology* examines the incentives for medical technology adoption and use, for hospital admissions, and for increasing or decreasing length of hospital stay under the new payment system.

ORGANIZATION AND BOUNDARIES OF THE CASE STUDY

The case study continues in chapter 2 by reviewing and analyzing what is known about geographic variations in hospital LOS in the United States, including a review of the magnitude of these differences and their trends over time. Chapter 2 also discusses how these variations are affected by population differences and reviews how various researchers have tried to explain them. Chapters 3 through 6 and appendixes A and B take a different approach. Each of these sections analyzes the medical literature relating to a specific clinical condition, attempting to ascertain whether research has established in a scientifically rigorous fashion that a particular LOS produces the best health outcome for that condition. Such an optimal LOS could serve as a standard against which geographic differences could be evaluated. Chapter 7 summarizes the findings across clinical areas and discusses future research needs and the policy implications of these geographic LOS variations for the Medicare and Medicaid programs.

This case study is specifically concerned with the relationship between hospital LOS and health outcome. Several important, related areas are not exhaustively reviewed. A comprehensive study of the cost implications of LOS variations and different methods of reducing LOS is beyond the scope of this study. Cost or charge data are discussed if they are part of studies that assessed outcome. But no attempt is made to measure directly how much various scenarios of LOS reduction might be worth. This is a complex question. Some of the problems one encounters in trying to address it have recently been reviewed (93).

A brief review of the relationship between LOS and health care costs may illustrate some of these complexities. Most hospital admissions incur greater costs at the beginning of a stay than toward the end when patients are nearing discharge and no longer require the intensity of diagnostic and therapeutic measures employed at the outset of their illnesses. If LOS is shortened by decreasing days at the end of a stay, the cost saving may be small. In fact, it may even be close to zero. At the end of a stay, most of the services consumed by patients represent fixed costs to the hospital: housekeeping, dietary, and laundry. Minimal nursing or ancillary services are provided. If LOS decreases, these fixed costs are still incurred by the hospital and must simply be distributed over a smaller number of patient days, thus increasing the average per diem room and board charge. In addition, if patients leave the hospital earlier, they may incur outpatient costs that they would not otherwise have. They may see their physicians more frequently or obtain more intensive home care. These factors too would reduce any savings realized as a result of early discharge. Further, if patients are discharged too early from a medical point of view, they may deteriorate at home and require readmission to the hospital, thus increasing total costs.

Another set of issues arises when one considers the likely effects of decreasing LOS for some patients on the rest of the hospital. If LOS were decreased for some patients, others might fill their beds, thus negating all or part of any savings that might be realized by the group that experienced fewer hospital days. For example, patients leaving early might be replaced by patients awaiting beds for elective surgery, thus reducing waiting time for these procedures. Since elective surgery patients will be likely to use more services per day on average than those discharged early would have had they stayed, total health care costs might easily rise, despite a reduction in LOS.

On the other hand, a decrease in LOS might lead to decreased occupancy without a rise in other admissions. Persistently low occupancy rates might cause some hospitals to close entirely or to convert wings to other, less expensive uses, such as long-term care. In this latter scenario, considerable cost savings might accrue. It is thus impossible to predict the effect on society's health care costs of decreasing LOS. The effect will depend ultimately on precisely how LOS is reduced, what the other relevant characteristics of the local health care market are, and whether any incentives have been established to facilitate the conversion of acute hospital beds to other uses.

Another important issue beyond the province of this case study is a full discussion of the most

appropriate place for various kinds of medical treatment. What sorts of procedures should be done in physicians' offices? Which kinds of patients must be treated in hospitals, and which could be managed safely as outpatients? These questions are important ones, with important economic implications of their own, but equally outside the realm of the present study. This decision rule also excludes clinical areas such as the treatment of drug addiction. This area has seen many studies, including some randomized clinical trials (RCTs), on the most effective LOS (37,97,134, 175). Because treatment of this kind ordinarily takes place outside the acute care hospital, it is outside the scope of this case study.

Each of the selected clinical areas was chosen because of the existence of at least one methodologically sound RCT that attempted to test the effect of changes in LOS on health outcomes. Each of these clinical areas is reviewed in depth. Some studies have assessed the effect of various modifications in medical practice and included measures of LOS in the assessment (38,85,105,146). Studies of this type have not been systematically sought. Each of the clinical studies reviewed here focused on the problem of the effect of changes in LOS on health. Evaluations of the efficacy of specific forms of medical technology or treatment are not within the purview of the present study. This review does, however, intensively analyze each clinical area in which researchers have sought to change LOS and to measure the effects of such a change on health.

EXECUTIVE SUMMARY

Eastern hospitals exhibit lengths of stay that are about 40 percent higher than western hospitals. These differences have remained remarkably consistent over the past 15 years. They are unexplained by differences among regions in age, sex, or race distributions. Current research has also been unable to demonstrate that differences in severity of illness across regions explain any of the variations. This possibility must remain at least somewhat open, however, since there has been little research at the most detailed clinical level to find subtle, but clinically important differences in case mix among regions within specific disease categories. Available evidence suggests that physicians in different regions treat patients with the same illnesses differently with respect to LOS.

As mentioned earlier, the potential economic significance of these LOS differences is very large. If all patients 65 years of age and older had experienced the West's LOS in 1980, those hospitalized in regions outside the West would have spent 21 million fewer days in the hospital, thus reducing total days in the hospital for this age group by 20 percent. How much of the potential savings could actually be realized depends entirely on how LOS is reduced, whether admission rates rise in compensation, and whether hospitals remove acute care beds from service in response to decreased occupancy. There is almost no research in this area.

Before designing new programs to reduce LOS, however, one must ask whether these regional differences in LOS are associated with differences in health outcomes. Are patients in the East harmed because they stay in the hospital longer than their western counterparts? Or are patients in the West suffering because they leave the hospital too early? Either, both, or neither of these possibilities may be true.

A great deal of research has addressed the association of LOS with factors such as hospital ownership, area hospital bed supply, teaching status, occupancy rate, and other hospital characteristics with varying results. Very little research has been done on the relationship between regional LOS differences and health outcomes. Very little attention has been devoted to ascertaining precisely how physicians manage the same kinds of illnesses in different regions of the country, trying to explain regional LOS differences by finding differences in physician practices.

This case study attempted to find data in the medical research literature clearly establishing a particular LOS for specific illnesses that produces

the best health outcome. Regions above the standard could be judged as having lengths of stay that were too long. Those below the standard could be judged too short.

Studies with scientifically sound methods were found in five clinical areas: acute myocardial infarction, elective surgery, low risk newborn deliveries, low birth weight infants, and psychiatric hospitalization. Studies in the first four areas uniformly concluded that shorter lengths of stay had no different outcomes from the more traditional, longer lengths of stay. None of the studies was large enough to rule out statistically the possibility that early discharge causes a small, but significant negative health impact. Many of the studies excluded the elderly. In the area of psychiatric hospitalization, the evidence is stronger that patients hospitalized initially for shorter periods do better than patients kept longer. Even in this area, however, the studies each assessed different patient groups and employed widely varying definitions of early (11 to 86 days) and late (24 to 179 days) discharge. Thus, the medical literature failed to establish clear LOS standards for any clinical condition.

Because the economic benefit of decreasing LOS to western levels is unclear and because the possibility of such a program having a negative health impact has not been excluded, the case for eastern lengths of stay is not definitive. Further research will be necessary to establish the relationship between length of hospital stay and health outcomes. The potential value of such research is very high.

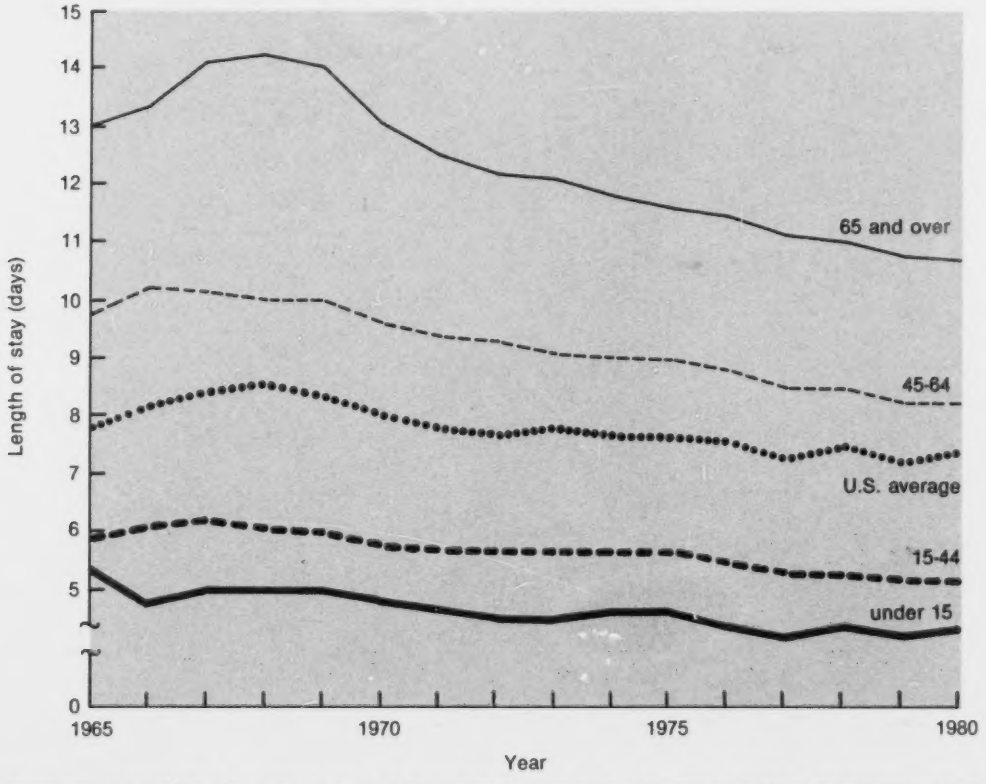
2.
Variations in Hospital Length of Stay

2.
Variations in Hospital Length of Stay

In general, average length of stay (LOS) in short-term, non-Federal hospitals has been falling since 1968 in the United States. Figure 1 shows the trends in LOS since 1965, the year in which the Medicare and Medicaid laws were passed, for the United States as a whole and for each of four age groups. The increase in LOS that followed the enactment of the Federal health insurance legislation and continued through 1968 in both the U.S. average and the elderly is the only dramatic departure from an otherwise virtually unbroken decreasing trend. The early years of the Medicare program also witnessed a rapid rise in the proportion of hospitalized patients that were elderly. While this proportion has risen continuously from 16 percent in 1965 to 26 percent in 1980, fully 40 percent of the increase took place between 1965 and 1968 (130). Since 1968 all age groups have shown decreasing lengths of stay; the elderly have decreased by 25 percent, the older adult group by 18 percent, the young adult group by 15 percent, children by 12 percent, and the combined U.S. average by 14 percent.

Given this pervasive downward trend in LOS, the stability of the geographic differences in LOS over time is remarkable. In 1980, the average LOS in the Northeast was 39 percent higher than in

Figure 1.—Age-Specific Trends in Hospital Length of Stay

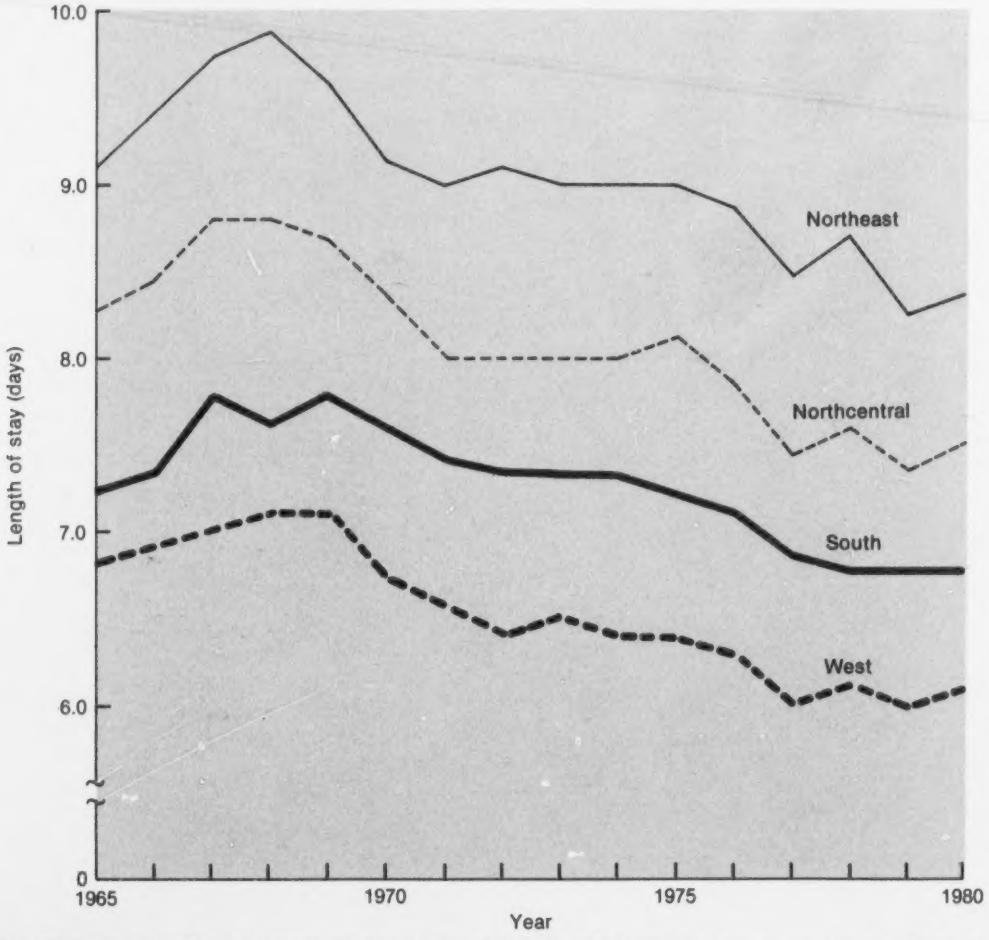


SOURCE: *Vital and Health Statistics*, series 13, Nos. 2, 10, 14, 17, 19, 23, 26, 31, 41, 46, 55, 60, 64 (Washington, D.C.: National Center for Health Statistics, 1967-82).

the West, the Northcentral was 23 percent higher, and the South 11 percent higher. Figure 2 shows how consistent these regional differences have been since 1965. Since the peak year of 1968, both the Northeast and the West have decreased in LOS by 14 percent, the Northcentral has fallen by 15 percent and the South by 12 percent.

An analysis of data collected by the Professional Standards Review Organization (PSRO) program reveals the same concentration of high LOS areas in the Northeast and low LOS areas in the West (74). Of the ten PSROs with the highest overall Medicare LOS in 1979, five were in New York, three in New Jersey, and one each in Illinois and Ohio. Of the ten with the lowest Medicare LOS, six were in California, one each in Oregon, Idaho, Washington, and Montana. PSRO Medicaid data from the same year are similar. Of the ten PSROs with the highest Medicaid LOS, two each were in Pennsylvania and North Carolina, and one each was in New York, New Jersey, Maryland, Virginia, Indiana, and Florida. Of the ten PSROs with the lowest Medicaid LOS, seven were in California, and one each in Idaho, Oregon, and Louisiana.

Figure 2.—Regional Differences in Hospital Length of Stay: Trends Over Time



SOURCE: *Vital and Health Statistics*, series 13, Nos. 2, 10, 14, 17, 19, 23, 26, 31, 41, 46, 55, 60, 64 (Washington, D.C.: National Center for Health Statistics, 1967-82).

Although this case study is concerned solely with LOS, it is relevant to ask the question: does this geographic pattern of low lengths of stay in the West mean that the West uses fewer hospital services per capita? The most commonly employed measure of hospital service use is total days of care per 1,000 population. This figure is the product of the average LOS and the admission rate per 1,000 in the region.

Table 1 shows data on admission and days of care rates per 1,000 population by region for 1980. The data show clearly that the West not only has the lowest LOS of any region but also the lowest admission rate. These two factors combine to give the West the lowest rate of use of total hospital days of any of the census regions. Some of the other regions do change their relative positions in the ranking of admission rates and days of care from where they stand with respect to LOS. For example, the Northeast has an admission rate that is slightly below the U.S. average, whereas the Northcentral has the highest admission rate. These factors contribute to the ranking of the Northcentral region as the one with the highest rate of use of hospital days of care. From another viewpoint, however, the data on total days of care are similar to those on LOS. The two regions with below average LOS (West and South) are the two regions with below average overall hospital use, as reflected in total days of care per 1,000. The Northeast and Northcentral regions, the areas with above average LOS, are the two regions with above average overall hospital use.

Table 1.—Admission and Days of Care Per 1,000 Population by Census Region in 1980

Region[a]	Admission rate (per 1,000 population)	Total days of hospital care (per 1,000 population)
Northeast	162	1,387
Northcentral	187	1,412
South	175	1,191
West	144	873
United States average	170	1,231

[a]Northeast = Maine, New Hampshire, Vermont, Massachusetts, Rhode Island, Connecticut, New York, New Jersey, Pennsylvania.
Northcentral = Michigan, Ohio, Illinois, Indiana, Wisconsin, Minnesota, Iowa, Missouri, North Dakota, South Dakota, Nebraska, Kansas.
South = Delaware, Maryland, District of Columbia, Virginia, West Virginia, North Carolina, South Carolina, Georgia, Florida, Kentucky, Tennessee, Alabama, Mississippi, Arkansas, Louisiana, Texas, Oklahoma.
West = Montana, Idaho, Wyoming, Colorado, New Mexico, Arizona, Utah, Nevada, Washington, Oregon, California, Hawaii, Alaska.

SOURCE: *Vital and Health Statistics*, series 13, No. 64, DHHS publication No. (PHS) 82-1725 (Washington, D.C.: National Center for Health Statistics, 1982).

In attempting to explain regional LOS differences, the first possibility that arises is that the demographic composition of the populations in the four census regions may be sufficiently different to account for all or part of the LOS differences. Of all the demographic variables, age has the strongest relationship to LOS. Figure 1 shows how rapidly LOS rises as a function of age, with the elderly spending 2.4 times as long in the hospital per stay in 1980 as those under age 15. In contrast, the average LOS for men in 1980 was 7.7 days, and for women it was 7.0 days. For whites the average LOS in 1980 was 7.3 days, the same as the U.S. average, and for all other races it was 7.5 days (129). Thus, it is conceivable that significant differences in age and sex (if not race) distributions could explain at least some of these geographic LOS variations.

Table 2 shows the 1980 age and sex distributions of hospitalized patients in each of the four census regions. On inspection, regional differences in these distributions appear to be minor. This observation proves to be correct. Table 3 shows the results of age and sex adjusting the regional figures for average LOS, using the direct standardization method. This method uses the entire U.S. population of hospitalized patients as the standard. The age- and sex-specific lengths of stay for each population subgroup of each region are then multiplied by the proportion that each subgroup represents in the standard population. These products are summed over all the age and sex subgroups to arrive at a figure that adjusts LOS for age and sex differences. Clearly, the effect of the age and sex adjustment is minimal. The West's LOS remained the same, the Northcentral and the South increased by 0.1 days, and the Northeast decreased by 0.1 days. Thus, while there are slight differences among the regions in the demographic characteristics of their hospitalized patients, these differences play a minimal role in explaining overall LOS variations. Gornick (60, 61,62) came to the same conclusion after a similar analysis of data pertaining to the Medicare population alone.

The lack of explanatory power of demographic characteristics has also remained constant over

Table 2.—Age and Sex Distributions of Hospitalized Patients by Census Region in 1980

Region	Percent male	Age group (percent of region's hospital patients)			
		Under 15	15-44	45-64	65 and over
Northeast	41.2	8.9	39.5	24.0	27.7
Northcentral	40.0	10.2	41.0	22.9	25.9
South	38.9	10.7	41.7	22.2	25.4
West	40.9	7.9	43.4	23.0	25.8
United States average	40.0	9.7	41.3	22.9	26.1

SOURCE: *Vital and Health Statistics*, series 13, No. 64, DHHS publication No. (PHS) 82-1725 (Washington, D.C.: National Center for Health Statistics, 1982).

Table 3.—Age and Sex Adjusted Length of Stay by Region in 1980

Region	Unadjusted LOS (days)	Adjusted LOS (days)
Northeast	8.5	8.4
Northcentral	7.5	7.6
South	6.8	6.9
West	6.1	6.1
United States average	7.3	—

SOURCE: Calculated from data in *Vital and Health Statistics*, series 13, No. 64, DHHS publication No. (PHS) 82-1725 (Washington, D.C.: National Center for Health Statistics, 1982).

time. If one examines age-specific lengths of stay for the four age groups displayed in figure 1 for the 15 years between 1966 and 1980 for each of the four census regions, one finds that the Northeast had the highest LOS for every age group in every year save one.* Thus, in 59 of 60 possible comparisons the Northeast showed the highest LOS. The same analysis reveals the West to exhibit the lowest LOS in 59 of 60 possible comparisons.

The possibility that case mix** differences among regions might account for some of the LOS variations is a much more difficult proposition to evaluate. Table 4 displays LOS data from the National Center for Health Statistics' (NCHS) 1980 Hospital Discharge Survey that are diagnosis specific for the 18 major diagnostic categories of the International Classification of Diseases: 9th Edition. Once again the data provide evidence of high LOS in the Northeast and low LOS in the West. In 13 of 18 categories, the Northeast is highest in LOS, and in 15 of 18 the West is lowest. It is also true, however, that the Northeast has slightly more patients in high LOS diagnostic categories than the other census regions. Table 5 gives the distribution of cases among the 18 diagnostic categories for the United States and the four census regions. In each of the three diagnostic categories with the highest average U.S. LOS (mental disorders, neoplasms, and diseases of the circulatory system), the Northeast has a greater proportion of cases than the average U.S. population. What effect do these case mix differences have on the difference between the Northeast and the West in LOS? Table 6 presents the results of a direct standardization of LOS by region using the U.S. distribution of cases as the reference population. The case mix differences described above have only a small impact, reducing the average LOS for the Northeast by 0.2 days.

Other data confirm this finding and extend it specifically to the Medicare and Medicaid populations. Gornick (61) found that for the Medicare population LOS for many specific conditions was highest in the Northeast and lowest in the West. She also found that adjusting New York's average Medicare LOS for California's case mix resulted in only a 0.1 day reduction in New York's LOS. Table 7 presents data from the PSRO program on Medicaid LOS for the 15 most common Diagnosis Related Groups in 1980. The same regional trends appear. In all 15 instances a western region exhibited the lowest LOS, while in 12 of 15 cases a northeastern region demonstrated the highest. Again, it appears that for every population examined, the Northeast has the highest LOS and the West the lowest for virtually all diagnoses.

*In 1978, the Northcentral region registered a LOS that was 0.1 days higher than the Northeast for patients 15 to 44 years of age.

**Case mix has been defined in various ways. In this case study, it refers to the relative frequency of various types of patients, reflecting different needs for hospital resources. There are many ways of measuring case mix, some based on patients' diagnoses or the severity of their illnesses, some on the utilization of services, and some on the characteristics of the hospital or area in which it is located.

Table 4.—Diagnosis-Specific Length of Stay by Region: 1980

Condition	LOS in days				
	U.S.	NE	NC	S	W
1. Mental disorders	11.6	11.6	13.4	9.8	10.7
2. Neoplasms	10.5	12.0	10.7	10.1	8.5
3. Circulatory disorders	10.0	12.4	10.2	9.2	8.1
4. Endocrine, nutritional, metabolic, and immunity disorders	9.6	11.8	9.7	8.9	7.7
5. Perinatal disorders	8.7	8.1	9.5	9.0	7.3
6. Musculoskeletal diseases	8.3	9.8	8.9	8.1	6.5
7. Skin diseases	8.0	9.5	7.8	7.4	7.8
8. Injury and poisoning	7.7	9.5	7.8	7.3	6.6
9. Hematologic disorders	7.2	8.8	6.9	6.8	6.0
10. Gastrointestinal diseases	7.0	7.9	7.2	6.6	6.2
11. Infectious and parasitic diseases	6.9	7.7	7.3	6.6	6.1
12. Congenital anomalies	6.6	6.8	7.5	6.0	5.9
13. Respiratory diseases	6.3	7.6	5.9	6.3	5.4
14. Genitourinary disorders	5.6	5.7	5.8	5.6	5.0
15. Diseases of the nervous system	5.4	6.3	5.5	5.4	4.5
16. Symptoms, signs, and ill-defined conditions	4.5	5.0	4.6	4.6	3.7
17. Supplementary classification (85% newborn deliveries)	3.7	4.3	4.1	3.4	3.0
18. Complications of pregnancy and childbirth	2.5	2.2	2.6	2.7	2.3
All conditions	7.3	8.5	7.5	6.8	6.1

Key: NE = Northeast, NC = Northcentral, S = South, W = West.

SOURCE: *Vital and Health Statistics*, series 13, No. 64, DHHS publication No. (PHS) 82-1725 (Washington, D.C.: National Center for Health Statistics, 1982).

Table 5.—Distribution of Cases by Diagnosis by Region: 1980

Condition	Region (percent of cases)				
	U.S.	NE	NC	S	W
1. Mental disorders	4.5	6.3	4.9	3.3	3.9
2. Neoplasms	6.5	8.0	6.6	5.5	6.7
3. Circulatory disorders	13.6	14.5	13.1	13.6	13.2
4. Endocrine, nutritional, metabolic, and immunity disorders	3.0	3.0	3.1	3.1	2.7
5. Perinatal disorders	0.2	0.2	0.3	0.2	0.3
6. Musculoskeletal diseases	5.9	4.6	6.6	5.7	7.0
7. Skin diseases	1.6	1.7	1.6	1.6	1.4
8. Injury and poisoning	9.5	8.4	9.3	9.3	11.8
9. Hematologic disorders	0.9	1.0	0.9	0.9	0.7
10. Gastrointestinal diseases	12.3	12.1	12.1	13.4	10.5
11. Infectious and parasitic diseases	1.7	1.7	1.6	1.9	1.6
12. Congenital anomalies	0.9	0.9	1.0	0.8	0.9
13. Respiratory diseases	9.1	8.1	9.2	10.1	8.2
14. Genitourinary disorders	9.5	9.4	9.2	10.4	8.3
15. Diseases of the nervous system	4.7	4.4	5.4	4.0	5.1
16. Symptoms, signs, and ill-defined conditions	1.7	1.3	1.7	2.0	1.6
17. Supplementary classification (85% newborn deliveries)	11.7	10.9	11.0	11.8	13.5
18. Complications of pregnancy and childbirth	2.7	3.4	2.5	2.4	2.6
All conditions	100.0	99.9	100.1	100.0	100.0

Key: NE = Northeast, NC = Northcentral, S = South, W = West.

SOURCE: *Vital and Health Statistics*, series 13, No. 64, DHHS publication No. (PHS) 82-1725 (Washington, D.C.: National Center for Health Statistics, 1982).

Blumberg (20) recently looked at this question from a different perspective. He studied whether the low LOS and hospital use rate in the West might be a reflection of a lower prevalence of morbidity in the population. He compared the NCHS Health Interview Survey measure of "restricted activity days" with LOS and admission rate data from the NCHS Hospital Discharge Survey. Restricted activity days are defined as days during which activity is decreased from usual because of an illness or injury that has been present for at least 3 months. He found that people in the West actually reported the highest rates of restricted activity days of any region, 37 percent higher than

Table 6.—Case-mix Adjusted Length of Stay by Region: 1980

Region	Unadjusted LOS (days)	Adjusted LOS (days)
Northeast	8.5	8.3
Northcentral	7.5	7.5
South	6.8	6.9
West	6.1	6.1
United States average	7.3	—

SOURCE: Calculated from data in *Vital and Health Statistics*, series 13, No. 64, DHHS publication No. (PHS) 82-1725 (Washington, D.C.: National Center for Health Statistics, 1982).

those in the lowest region, the Northcentral. After standardizing LOS by these morbidity measures, he found that western lengths of stay were lower than expected while those in the Northeast and Northcentral were higher than expected. Thus, adjusting for differences in population morbidity actually widened regional LOS differences. While one may argue that the measure of morbidity used was insensitive or subject to biased reporting because of regional population differences in interpretation, the study does establish that regional differences in this particular morbidity measure do not account for regional LOS variations.

Two recent PSRO studies have examined the relationship between case mix or severity of illness and LOS. In both of them, an eastern PSRO and a western PSRO paired themselves and carefully scrutinized data from medical records for two specific kinds of patients in an effort to explain the differences between them in LOS. In one study, the Utah PSRO (UPSRO) and the Central Massachusetts PSRO (CMPSRO) analyzed LOS for their patients with myocardial infarction (MI) and cholecystectomy. Table 8 presents LOS data for these two PSROs and these two conditions. The data demonstrate the typical pattern of lower western LOS for each diagnosis and each insurance subgroup.

In the MI study (24), patients were included only if they had had a documented MI and were Federal beneficiaries (Medicare and Medicaid patients). Patients from small hospitals (less than 1,500 discharges per year) were excluded. All of the patients were classified into severity of illness categories to distinguish those with uncomplicated MIs, those with MIs with congestive heart failure, and those with cardiogenic shock. There were no differences between the two patient populations in proportion of patients in each severity class or in class-specific mortality rates. However, LOS for each severity class was longer in the CMPSRO populations.

This study has some important limitations, including the fact that the UPSRO study population represented patients from 3 months in 1979,

Table 7.—Highest and Lowest DHHS Regions for Medicaid Length of Stay in 1980 by Diagnosis Related Group (DRG)

Description and number of DRG	Number of cases	Highest region	LOS (days)	Lowest region	LOS (days)
1. Normal Mature Newborn (318)	357,633	II[a]	5.2	IX	3.3
2. Normal Delivery (278)	208,641	V	3.3	IX	2.2
3. Complicated Delivery (281)	159,092	V	3.8	IX	2.8
4. Functional Intestinal Disorder (206)	58,990	II	5.5	X	3.6
5. Schizophrenia (89)	44,206	II	20.5	IX	11.5
6. Seizures, syncope, chest pain, or epistaxis with secondary diagnosis (323)	30,236	II	7.3	X	4.0
7. Electrolyte disorder (333)	27,016	II	9.2	X	5.2
8. Urinary symptoms (328)	20,860	II	7.2	X	4.3
9. Fracture with major operation, including hip arthroplasty (348)	16,792	II	18.9	IX	12.0
10. Diabetes (75)	15,276	II	12.0	X	6.8
11. Congestive heart failure (132)	15,996	II	11.6	X	6.7
12. Ischemic heart disease without operation (124)	12,095	II	8.5	X	4.8
13. Acute Myocardial Infarction (121)	11,578	II	14.4	X	9.8
14. Pneumonia over age 30 (167)	11,066	II	12.0	X	7.8
15. Emphysema without operation (176)	10,941	III	14.4	X	6.9

[a]Region II = New York, New Jersey; Region III = Pennsylvania, Delaware, West Virginia, Virginia, District of Columbia, Maryland; Region V = Illinois, Indiana, Ohio, Wisconsin, Michigan, Minnesota; Region IX = California, Nevada, Arizona, Hawaii; Region X = Washington, Oregon, Idaho, Alaska.

SOURCE: *PSRO Data Reporting and Analysis System* (Baltimore, Md.: Health Standards and Quality Bureau, 1982).

Table 8.—1978 Length of Stay Data for Utah and Central Massachusetts PSROs by Source of Payment

Condition and payment source	LOS in days UPSRO	CMPSRO
Myocardial infarction:		
Medicare	12.9	15.7
Medicaid	11.2	14.7
Cholecystectomy:		
Preoperative:		
Medicare	2.5	4.2
Medicaid	1.7	3.1
Total stay:		
Medicare	10.8	15.2
Medicaid	8.0	10.5

SOURCE: *PHDDS Report Series*, "1978 Medicare/Medicaid Split Report" (Baltimore, Md.: Health Standards and Quality Bureau, 1980).

whereas the CMPSRO population was a sample of 1978 patients. In addition, patients were included in the study based in part on electrocardiogram readings performed by different physicians who did not employ uniform criteria. Despite these limitations, however, the study is significant as one of the very few that have attempted to discover what clinical factors underlie regional LOS differences.

In the cholecystectomy study (25), data were collected prospectively in both PSROs from February to June 1980. As in the MI study, patients were classified into disease stages according to previously developed severity of illness criteria. Three categories of patients were identified: those without gallstones (Stage I), those with stones (Stage II), and those with severe conditions such as cholangitis, perforated gallbladder, or emphysema (Stage III). There were more patients in the most severe class in the CMPSRO population (22 v. 14 percent), fewer patients in Stage I (6 v. 11 percent), and about the same proportion in Stage II (72 v. 75 percent). LOS data were given only for Stage II patients, where LOS was 3.1 days longer for CMPSRO patients.

Four other interesting conclusions emerged from this study. First, patients initially admitted to the medical service in central Massachusetts had a far longer LOS than those admitted first to the surgical service (18.2 v. 10.4 days). The medical patients in Utah also had a longer LOS, but the difference was far less (11.2 v. 8.9 days). In both cases, 70 percent or more of this difference occurred before the operation. Thus, internists in central Massachusetts took almost 4 days longer than their Utah counterparts to make the diagnosis of cholelithiasis and arrange for surgery. Second, there were no differences in the incidence of postoperative morbidity, rate of common duct exploration, or the performance of additional procedures between the two PSROs. All of these factors increased LOS in both populations, but their relative rate of occurrence was the same.

Third, while there was no difference between the two groups in the day that patients first resumed oral feeding, there was a significant difference in the day of first ambulation. In Utah, 80 percent of patients were ambulatory the day after surgery, while only 35 percent of the central Massachusetts patients were so treated. This difference in medical practice may have contributed to the 1.6 day longer postoperative LOS in central Massachusetts. Finally, the study analyzed the difference in distribution of its patients according to the anesthesia risk code assigned by the individual patients' anesthesiologists. This measure may be viewed as an independent severity of illness classification. In this analysis, the Utah patient population had a slightly greater anesthesia risk than the CMPSRO population. This finding indicates that within the Stage II severity class, CMPSRO's greater LOS cannot be attributed to greater severity of illness. Thus, this study is consistent with the MI study in suggesting that severity of illness differences do not explain higher eastern lengths of stay. It is also significant in that it suggests some differences in medical and surgical practice that may contribute to these LOS differences.

A similar set of studies was carried out by two PSROs in the Baltimore area (BPSRO) and the Multnomah (Portland, Oreg.) Foundation for Medical Care (MFMC). The Baltimore City PSRO (BCPSRO) and the Central Maryland PSRO (CMPSRO) combined to perform medical audits with MFMC on cataract and cardiac patients. The first audit, on cataract patients, was selected because the BPSROs and MFMC exhibited widely diverging lengths of stay for cataract surgery in 1977. Medicare patients stayed 3.8 days in the hospital for cataract surgery in MFMC, 7.1 days in CMPSRO, and 7.2 days in BCPSRO. Analysis of their combined hospital discharge abstract data for

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1978 revealed that these differences persisted after controlling for sex, race, discharge status, and secondary operations. A small part of the variation was found to be due to the fact that some Baltimore patients had bilateral lens extractions but no MFMC patients did. Wide variations were found among physicians and hospitals in both MFMC and BPSRO in average LOS for cataract patients.

The medical audit was performed using identical criteria on a sample of BPSRO and MFMC patients from 1980. The audit found that 20 percent of BPSRO patients had preoperative stays of more than 1 day and that only 14 percent of these long stays passed the appropriateness criteria. In addition, 42 percent of BPSRO patients had lengths of stay greater than 4 days, and only 19 percent of these stays passed the appropriateness criteria for postoperative LOS. A 6-week followup assessment was requested from one-third of the physicians. Inhospital and 6-week rates of complications were comparable in the two areas (7).

Three cardiac diagnoses were studied in the second audit: myocardial infarction, congestive heart failure, and angina. LOS in MFMC was 4 to 7 days shorter than in the BPSROs for all of these diagnostic categories after controlling for age, sex, race, pay source, surgery, and multiple diagnoses. The audit established criteria for making these three diagnoses and for determining severity of illness. The results showed that coding accuracy was comparable across the two areas. In addition, Baltimore patients stayed 3 to 11 days longer than MFMC patients in each severity class of each of the three diagnostic categories. Once again, large differences were also found among hospitals within each PSRO area (7).

These studies are consistent with the two studies from Utah and Massachusetts in demonstrating that demographic and case mix differences cannot account for large LOS variations between east and west. Unlike the previously discussed PSRO studies, the Baltimore/Portland studies did not attempt to discover what was different about physician management in the two areas. One can also question the reliability of physician self-reports of complication rates. None of the four PSRO studies adequately assessed the outcome of the treatment rendered to their study patients. These investigations do, however, represent the best attempt to date to study in clinical detail an eastern and western patient population trying to find severity of illness differences. All four studies used carefully designed criteria to define graded classes of severity within the disease categories analyzed. All four found that large differences in LOS remained after controlling for severity of illness.

Complementing these PSRO studies on specific clinical conditions is the Stanford Institutional Differences Study, one part of which addressed regional differences in LOS and case mix from a more global perspective. This study (51) examined the records of 603,000 patients from 17 hospitals over the 4 years from 1970 to 1973. The hospitals were a representative sample from among those participating in the Professional Activities Survey (PAS) administered by the Commission on Professional and Hospital Activities. Using PAS abstract data, the study measured intensity of service, LOS, and outcomes. Intensity of service was measured as a composite variable that included measures of numbers of laboratory and X-ray procedures, surgical procedures, and transfusions; use of physical therapy or intensive care; and the number of different types of drugs used. Each of these components was weighted by the relative proportion of patient charges each area consumed in order to construct the single composite score for intensity of service. LOS was measured simply as the number of days of hospitalization. Outcome was measured as proportion of patients who died prior to discharge. Each of these measures was standardized for differences among hospitals in admitting diagnosis, additional diagnoses, age, sex, number of surgical procedures, and complications. This was done by pooling all 603,000 records and constructing regression equations for each diagnostic group to predict the values, in turn, of the composite intensity of service variable, LOS, and proportion of deaths. Each hospital's actual experience was then compared to its "expected" experience.

The results are striking. Hospitals that provided more services and kept their patients fewer days

had better than predicted outcomes. Each of these factors had an independent and statistically significant effect. The combined effect of the two factors partitioned the 17 hospitals almost perfectly into performance subgroups. The four hospitals with the lowest standardized death rates all provided greater than expected intensity of service and shorter than expected lengths of stay. The five hospitals with the highest standardized death rates all provided less than expected intensity of service and longer lengths of stay. The remaining hospitals were intermediate. In a two-way analysis of variance, intensity of service and LOS explained 77 percent of the variance in mortality rates with all variables standardized.

Even more significant for the present study was the finding that virtually all of the variation in LOS could be explained by regional location of the hospitals. The usual pattern was found. The West and South had lower than expected standardized mortality rates while the North's was higher than expected. The West also provided a greater intensity of service than expected, while the North and South provided fewer than expected services. These results are summarized in table 9. The study then examined the effects of LOS and intensity of service within regions. The study found little variation in LOS within region but found that the intensity of service variable still predicted outcome within regions: the greater the intensity of service, the better the mortality rate.

This study is unique in its attempt to associate regional LOS differences with outcome differences. Several aspects of the study require further comment. First, after an extensive standardization process that controlled for demographic and case mix differences among patients, regional LOS differences in this sample were not only preserved, but enhanced. The average LOS difference between North and West before standardization was 2.1 days, while after standardization it was 2.4 days. Thus, this study provides further evidence that demographic and case mix differences do not account for regional LOS differences. The study also suggests one very general way in which eastern, western, and southern physicians may differ in their patient management practices. After adjusting for differences in patient characteristics, the study found that western patients received more services than expected, eastern patients received fewer than expected, and southern patients received even fewer. Table 9 shows these results. The study did not present data on the components of this difference, so it is not possible to analyze what this difference means for specific kinds of patients. One cannot determine the clinical meaningfulness of these differences in intensity of service. However, this study does provide convincing evidence of definite, if nonspecific, regional differences in patient management practices.

The most interesting finding of the study from the perspective of the present analysis is the association of lower lengths of stay with better outcomes. While this finding is suggestive, three important considerations mitigate its impact. The first is that the magnitude of the mortality difference is quite small. Table 9 reproduces the crude and standardized mortality rates for the hospitals in the sample by region. The data show that after adjusting for demographic and case mix differences among patients, regional mortality rates cluster very closely about their expected

Table 9.—Regional Differences in Length of Stay, Intensity of Service, and Mortality Rates From the Stanford Institutional Differences Study

Region	Number of hospitals	Length of stay (days)		Intensity of service score		Mortality (percent of deaths at discharge)	
		Crude	Standardized[a]	Crude	Standardized	Crude	Standardized
North	9	9.3	0.8	46.3	-1.0	4.0	0.3
South	3	7.1	-0.5	42.0	-3.7	2.9	-0.4
West	5	7.2	-1.6	52.2	1.6	2.6	-0.3
p value for one-way analysis of variance		0.0007	0.0001	0.095	0.085	0.073	0.012

[a]Negative signs indicate values less than expected after standardization; positive signs indicate values greater than expected.

SOURCE: Adapted from table 5, A. B. Flood, W. Ewy, W. R. Scott, et al., "The Relationship Between Intensity and Duration of Medical Services and Outcomes for Hospitalized Patients," *Med. Care* 17:1088-1102, 1979.

values. The West is only 0.3 percent below expected and the North only 0.3 percent above.

The second consideration is the method chosen for standardization—using pooled PAS discharge abstracts. Hospital discharge abstracts, such as the PAS system, have repeatedly been found to be unreliable reporters of diagnostic and procedural information. The Institute of Medicine (IOM) studied private abstracting services (including PAS) in 1974 and found that principal diagnosis was incorrect 35 percent of the time and that principal procedure* was incorrect 27 percent of the time (128). Moreover, there is considerable room within these diagnostic categories, even when accurately reported, for large differences in severity of illness. It is true that the study did use other measures from the PAS abstract as proxies for severity of illness, including number and severity of surgical procedures and secondary diagnoses. However, these are likely to be subject to the same reliability problems as principal diagnosis and procedure. Nor can they entirely reflect severity of illness differences among patients in the same general diagnostic categories. For example, the Utah-Central Massachusetts PSRO study showed that MI patients who showed any degree of congestive heart failure but no signs of shock on admission (i.e., an intermediate level of severity) experienced more than twice the mortality of those admitted with no signs of heart failure (32 v. 14 percent). This kind of difference in severity of illness could not be discerned from a hospital discharge abstract, and therefore, could not be controlled for in the Stanford study. Therefore, it is possible that the small differences in mortality that remained after the standardization method used in the study was carried out could still be explained by severity of illness differences not measured by PAS abstract variables.

The third and most important consideration is that the outcome measure assessed only inhospital mortality. Since the most important potential danger of short lengths of stay is the possibility that early discharge may lead to clinical deterioration and death after discharge, inhospital mortality is an incomplete outcome measure.

Despite these concerns, one must still emphasize that this study represents the best effort to control for regional differences in case mix and the only effort to assess the relationship of regional LOS variations to any sort of outcome measure. It is consistent with the data presented earlier that document large variations in regional LOS unexplained by demographic or case mix differences among patients. Making the most of its data base, the study documented small, but statistically significant differences in hospital mortality among regions, with the better outcome associated with shorter lengths of stay. Unfortunately, the inadequacy of hospital mortality rate as a measure of outcome when assessing LOS differences lessens the significance of this finding.

What then can be concluded concerning the possibility that differences in severity of illness might explain regional LOS variations? First, all of the available studies and data are consistent in failing to document any significant reduction in regional LOS differences by case mix adjustment. Second, with the exception of the PSRO studies discussed previously, there has been no attempt to scrutinize carefully the different patient populations for severity of illness differences not revealed in differences among diagnostic categories. The Stanford study did try to go somewhat beyond these bounds, and its limitations have already been addressed. The fact that no comprehensive study has been done that assessed regional severity of illness differences of the kind reported in the PSRO studies is a major deficiency in the existing literature. Until this deficiency is remedied, the possibility will remain open that some of the regional differences in LOS might be attributable to regional differences in severity of illness among hospitalized patients. Having admitted this possibility, one must stress that, given the broad similarity of populations across the four major census regions, it is unlikely that severity of illness differences large enough to explain the considerable regional LOS variations could exist.

If there is little evidence that demographic or case mix differences explain regional LOS variations, what factors are responsible? While a large

*Although not reported in the PAS study, IOM found in similar studies of the Medicare and the National Hospital Discharge Survey systems that, while patients undergoing no procedures were identified correctly about 80 percent of the time, of those patients undergoing procedures, the principal procedure was incorrectly recorded 36 to 43 percent of the time (126,127).

number of studies have sought associations between LOS and other variables, few have explicitly addressed regional differences. The studies that have tried to explain differences in hospital LOS have examined vastly different samples of patients and hospitals. Table 10 provides a representative, but not exhaustive, list of the different kinds of samples that are reflected in the literature. Table 11 lists the factors that have and have not been shown to be statistically significantly associated with LOS differences in these studies.

For the purposes of this review, all of these studies are deficient in three crucial ways. First, none of them addresses the issue of *regional* LOS differences. Thus, it is unclear whether any of the factors identified in these studies is an important factor in explaining regional variations. Second, none of these studies attempts to discover differences in physician practices that might account for LOS differences. These factors have been left entirely out of account. It is only in the PSRO and Stanford studies discussed above that this issue has begun to be addressed. Third, none of these studies looks carefully at severity of illness differences. Where case mix differences are considered, most often only primary diagnosis are used to adjust for such differences. Some studies consider the presence or absence of secondary or multiple diagnoses. But none of them consider the variation in severity of illness that occurs within diagnostic groups. Again, the PSRO and Stanford studies cited previously are unique in their examination of this issue.

While the main body of the literature on hospital LOS and its associations may not be very useful in this analysis, one study does shed some additional light on regional LOS variations. The study by Gornick (61,62) has already been cited in other contexts in this review. She presents the results of a multiple regression study that was performed using average LOS for Medicare patients in 1979 as the dependent variable. The PSRO area was the unit of analysis. She studied the effect of region as a dummy variable after differences in demographic and supply variables had been controlled for. The study found that occupancy rate,

Table 10.—Populations Studied for Length of Stay Associations

1. Six hospitals in Sweden, pediatric enteritis, 1968 (166)
2. Scottish surgeons, eight procedures, 1974 (31)
3. Cholelithiasis patients, one Australian hospital, 1973-79 (89)
4. Winnipeg General Hospital (155)
5. Long-stay obstetrical and gynecological patients, Edmonton hospital (144)
6. Medicaid and Blue Cross patients, Maryland, 1967-77 (170)
7. Blue Cross/Blue Shield patients, Michigan, 1976 (94)
8. Patients in matched Veterans Administration and non-Federal hospitals (47)
9. Cesarean section patients, University of Virginia Hospital, 1978 (44)
10. Toronto West Hospital, 1974 (167)
11. Four Boston area hospitals, 1964 and 1974 (159)
12. Teaching hospital in Pittsburg, 1970-71 (104)
13. Two Baltimore hospitals, 1968-70 (132)
14. A Nottingham hospital, 1970 (181)
15. Two Washington, D.C., hospitals, 1973 (156)
16. Cataract patients, Washington, D.C., 1977-79 (185)
17. Patients with diabetic ketoacidosis, University of Missouri Hospital (67)
18. 23 New York hospitals, Medicaid patients, 1972 (139)
19. Two London teaching hospitals, 1972-75 (50)
20. 22 Pittsburg hospitals, 1963 (145)
21. Surgical patients, University of Virginia, 1973-74 (63)

NOTE: See Reference list for complete citations of studies in table.

Table 11.—Factors Found To Be Associated and Unassociated With Length of Stay

Factors associated with increased LOS:
Comorbid conditions (50,104,166,185)
Complications (44,50,63,144)
Medicaid insurance (170)
Use of consultations (94)
Federal hospital ownership (47)
Turnaround time for laboratory tests (47)
Adverse drug reactions (167)
Number of surgical procedures (104)
Emergency admissions (104)
Teaching hospital (145)

Factors associated with decreased LOS:
Teaching hospital (31)
Proportion of foreign medical graduates on staff (47)
Occupancy rate (47)
Private room use (104)
Close association with chronic disease hospital (132)
Appropriate drug prescribing (98,99)
Primary physician gatekeeper experiment (120)
Presence of outpatient clinic (145)

Factors not associated with LOS:
Distance patient lives from hospital (166)
Social disadvantages (166)
Occupancy rate (104)
Insurance status (104)
Continuity of care (181)
Teaching hospital (156)
Specialist v. generalist care (67)
Health maintenance organization delivery care (110)

NOTE: See Reference list for complete citations of studies in table.

hospital bed supply, and percent of total population living in Standard Metropolitan Statistical Areas were all positively and significantly associated with higher LOS. Nursing home bed supply was negatively correlated with LOS. Age, percent female, and percent non-white were also positively correlated with LOS, although only the latter two were statistically significant. Even after regional differences in these factors were taken into account, significant regional differences in LOS persisted. Dummy variables representing the difference between the West and each of the other three regions were tested. Those for the Northeast and Northcentral were highly significant, while the one for the South was not.

Two aspects of this study deserve further comment. First, no attempt was made to adjust for case mix differences. Thus, this study cannot further clarify the extent to which these case mix differences explain regional LOS variations. Second, from the perspective of the current analysis, it is not clear that one would want to adjust LOS differences for differences in area supply or personnel characteristics. Before assessing the magnitude of any regional LOS differences, it is appropriate to remove the effects of differences in variables that might contribute to LOS differences considered to be medically justifiable. Therefore, adjustments should be made for differences in patient demographic characteristics and case mix. However, it is not clear that differences in bed supply or occupancy rate result in medically justifiable differences in LOS. Indeed, any impact on LOS they may have is likely to be medically inappropriate. Areas with relatively too many hospital beds and low occupancy rates may, for example, be induced to keep patients in the hospital longer than necessary. If their lengths of stay are higher for these reasons, then it is not appropriate to adjust for the effects of these variables. The relationships of LOS to supply and personnel variables may be interesting from an econometric viewpoint, but they have little relevance to the question of whether Northeast lengths of stay are "too high" or those in the West "too low."

Thus far, it has been demonstrated that regional differences in hospital LOS cannot be explained by differences in patients' demographic characteristics. In addition, there is no evidence that case mix differences explain these variations. It has been noted, however, that a comprehensive clinical study of regional case mix differences has not been done. Therefore, the first key question posed at the outset of this investigation can probably be answered in the negative. Regional LOS differences are probably not simple functions of population differences in demographic characteristics or case mix.

There are only fragmentary data with which to address the remaining two key questions. The two sets of PSRO studies discussed above provide some documentation that eastern and western physicians manage similar kinds of patients differently. One example of this phenomenon is the difference in cholecystectomy patients' first day of ambulation in Utah and central Massachusetts. The Stanford study documented that the western patients in its sample received more services than the eastern and southern patients. However, it is simply not known in any clinical detail how eastern and western physicians vary in their patient management of a variety of similar conditions. There has been no comprehensive study of differences in eastern and western patient management techniques and how any such differences might account for regional LOS variations. Because data to answer this important question are largely absent, the answer to the third key question—how patient management differences affect outcome—must also remain presently unknown. No study has even attempted to measure regional differences in outcomes of hospital care in a way that would allow an assessment of the medical implications of regional LOS differences.

Does this lack of information mean that no conclusions can be drawn regarding the appropriateness of the large regional differences in LOS? Are there no other data that might illuminate the problem? While there have been no adequate studies of the relationship between regional LOS differences and health outcomes, a large number of studies have been done that examine the health consequences of differing lengths of hospital stay for the same clinical condition. This body of literature may be of help, assuming that practice patterns cause the LOS variations.

Since it has been demonstrated that differences in population demographics and case mix are

unlikely to be important factors in explaining regional LOS differences, it thus appears that physicians treat similar kinds of patients differently in different regions of the country. If the medical literature clearly establishes that for a particular condition a 10-day LOS has the best health outcome, regions with lengths of stay for this condition of more than 10 days could be judged as keeping their patients too long while those under 10 days would be providing too little hospital care. In performing this analysis, one must be prepared for the possibility that all regions may exhibit current lengths of stay that are either above or below an optimal LOS determined from the literature. One must also be prepared for the more likely possibility that an optimal LOS cannot be inferred from the literature. However, because it may shed some additional light on the problem of regional LOS differences, this kind of analysis may assist health policy decisions in this area.

The remainder of this case study reviews the medical literature that describes the relationship between LOS and health outcomes. A review of this literature disclosed five clinical areas in which methodologically sound studies have been performed: acute myocardial infarction, certain elective surgical procedures, low risk newborn deliveries, low birth weight infants, and psychiatric hospitalization. The studies in each of these areas are carefully examined to discover what is known in each clinical condition about the health consequences of differing lengths of stay.

Before proceeding to examine each of these clinical conditions independently, it is important to consider conceptually the ways in which LOS and health outcomes might be related. One must first recognize that the duration of a hospital stay is not a directly manipulable factor in patient management. If LOS is shortened, then treatment schedules must be altered in very specific ways. Some treatments must be foregone, others changed, and others shortened in duration. For example, if a patient with pneumonia is sent home early, one might have to decrease the number of days during which intravenous antibiotics are given. The MI patient may be required to get out of bed and walk sooner. The surgical patient might have to begin a normal diet sooner and perhaps leave the hospital with his or her sutures still in place.

Each of these changes from preexisting practice may have negative health consequences. The pneumonia patient might experience a relapse because potent intravenous therapy was discontinued too soon. The MI patient might suffer an extension of the infarct, because too much work was required of the recuperating myocardium. The surgical patient might experience a wound infection or dehiscence if the wound is not watched closely and cared for antiseptically. In general, the potential negative health impact of decreasing LOS would flow from the failure to provide some aspect of treatment that is effective in improving the health outcome of a particular condition.

Hospital stays may also be beneficial in protecting patients from the adverse health effect of factors present in their home environments during especially vulnerable periods in their convalescence. Family conflicts may adversely affect recuperating MI patients. While compliance with therapeutic regimens can be assured to a great degree in hospital inpatients, the same is not true for those discharged. Lack of compliance may have particularly significant adverse effects early in convalescence. Early discharge of tuberculosis patients has been criticized as a possible danger to public health (133).

On the other hand, hospitals can be hazardous to one's health. Complications of hospital treatment are many, including nosocomial infections, adverse drug reactions (which may also occur with outpatient treatment, but those that occur in association with inpatient intravenous drug use are more frequently very serious), complications related to procedures, and others. Clearly, one's probability of experiencing one of these adverse effects of hospital care increases directly with one's exposure; the greater the LOS, the greater the chance.

It should be clear, therefore, that the health effect of decreasing LOS cannot be determined a priori; it is an empirical question that can be addressed only by careful research. The kind of study best able to illuminate this issue is one in which the patient population is carefully described and in which a clearly defined set of treatments is modified in order to effect a shortened LOS. Such a study must also measure a set of outcomes

plausibly related to the treatments that have been altered. From a methodological viewpoint, the randomized clinical trial (RCT) offers the best chance at measuring the effects of such an experiment in an unbiased fashion. This case study, therefore, pays special attention to RCTs.

This case study also excludes studies if changes in clinical practice render them obsolete. One example is an RCT done in the late 1950's on early ambulation of patients with upper gastrointestinal tract bleeding that was done prior to the advent of flexible fiberoptic endoscopy and cimetidine (138). Another example is the question of the value of bed rest in the treatment of hepatitis. A series of studies, including some RCTs on military populations, has failed to demonstrate any benefit of bed rest in the treatment of this condition (32,91,142,176). But treatment for hepatitis now ordinarily takes place on an outpatient basis. Because hospital treatment is usually reserved only for patients who experience serious complications of their disease, this subject was considered outside the scope of this study.

One final point should be borne in mind. Because the effect of changes in treatments on health outcomes is so dependent on precisely which treatments are altered, in precisely what manner, in which kinds of patients, one cannot generalize the results of one study in a particular clinical area to another. Indeed, because of the many ways in which study populations can be defined even for a single condition, one may not be able to compare studies of the same condition very well. The general proposition of the relationship of LOS to health outcomes must be investigated by studying each medical condition of interest by itself.

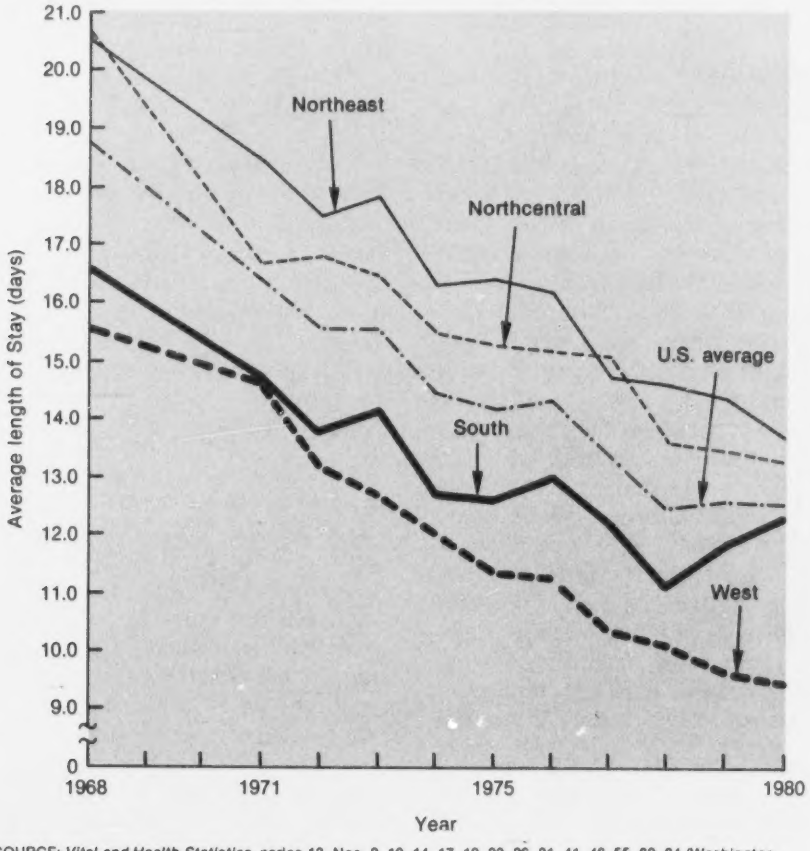
3.
Length of Stay and Outcome: Myocardial Infarction

3. Length of Stay and Outcome: Myocardial Infarction

Myocardial infarction (MI) is the clinical condition most often studied in the attempt to find a relationship between length of stay (LOS) and health outcome. These studies are reviewed in depth in appendix A and are summarized here. Virtually all physicians prescribed prolonged bed rest (usually 6 to 8 weeks) for patients with MIs through the 1940's (109,112,123,179). By the early 1950's, a few centers were trying earlier ambulation and discharge (8,10,27,43,70,90,107,108).

Beginning in the 1960's and accelerating into the 1970's, increasing numbers of research studies of early ambulation for MI patients were published. Associated with the appearance of these studies has been a rapid decline in the United States LOS for MI patients. Figure 3 describes the extent of this decrease by region since 1968. Over the 12 years prior to 1980, LOS for MI patients in the United States fell by 33 percent, compared with 14 percent for LOS for all patients.

Figure 3.—Regional Trends in Length of Stay for Myocardial Infarction



SOURCE: *Vital and Health Statistics*, series 13, Nos. 2, 10, 14, 17, 19, 23, 26, 31, 41, 46, 55, 60, 64 (Washington, D.C.: National Center for Health Statistics, 1967-82).

The literature contains three different kinds of studies. The first group comprises studies that analyzed clinical data retrospectively trying to explain variations in treatment practices or to identify characteristics of low-risk MI patients who might be candidates for early discharge. Studies in the second group reported the effects of early ambulation and discharge programs for MI patients without providing any control data. Studies in the third group provided control data, including some that were randomized clinical trials (RCTs). The data provided by each of these groups of studies are summarized in turn, focusing on the third group. They are reviewed in detail in appendix A.

Several studies have attempted to examine variations in individual physician practices with respect to LOS for MI patients (46,76,137,147,177) with no conclusive results aside from demonstrating large variations in LOS among physicians. Another series of studies has attempted to define criteria that would identify low-risk MI patients (111,117,158,163,165,182,183). Of all these sets of criteria, the one most often studied is the one developed by McNeer and colleagues at Duke University. They first observed in 1975 (117) in an analysis of 522 consecutive patients with documented MIs, that patients who had suffered a serious complication *after* the first 4 hospital days also had had one *during* the first 4 days. The complications identified as serious were: death, ventricular fibrillation or tachycardia, second or third degree AV block, pulmonary edema, cardiogenic shock, persistent sinus tachycardia or hypotension, atrial flutter or fibrillation, and extension of infarct. They also found that of the patients without complications in the first 4 days, there was no inhospital mortality during an average LOS of 17 days. The 6-month mortality was 8 percent. This compared with an inhospital mortality of 14 percent and a cumulative 6-month mortality of 19 percent in the complication group. In their original series, patients with uncomplicated MIs made up 51 percent of the total MI population.

The Duke criteria have been replicated in three retrospective studies with similar results (158,165, 183). However, one cannot conclude from these studies that patients without complications in the first 4 hospital days following an MI can be safely discharged after that time. All of the studies thus far mentioned were retrospective; no attempt was made to actually discharge the low-risk patients earlier than their physicians at the time thought appropriate. It is entirely possible, then, that earlier ambulation in preparation for earlier discharge would have proved harmful. The fact that all of these studies used almost identical criteria and found similar results lends added weight to the potential reliability and validity of these criteria as predictors of good prognosis and, therefore, of candidates for early discharge. Better data are needed, however, in order to establish this proposition conclusively.

Eight studies report results from uncontrolled attempts to mobilize and discharge uncomplicated MI patients early (2,22,26,27,35,53,173,174). It is difficult to draw definitive conclusions from these studies. First, none of them was performed in the United States. Second, the studies excluded significant, but varying portions of the population. Four excluded women, and two excluded the elderly. Third, the definitions of early ambulation and discharge varied among the different studies. But the most serious difficulty with these studies is the absence of any comparison data. Without carefully selected control groups, one does not know whether the patients who ambulated and left the hospital early would have done better or worse if treated for a longer time in the hospital.

Of those studies that have provided data from control groups, five reported results for controls selected in ways other than by random assignment (21,66,69,102,116). All of these studies developed protocols for identifying and discharging early low-risk patients with uncomplicated MIs. Their methods of selecting controls, however, prevents one from concluding that the differences they report between study and control patients are due solely to the early discharge program. One (66) allocated patients to study and control groups based on which of two physicians cared for the patients. One (69) allocated patients according to which of two hospitals was the site of treatment. A third (21) compared patients who left within

10 days to those who remained after that time. The fourth study (102) is unclear about how its controls were selected.

The fifth study (116) in this group is a prospective study conducted by McNeer and colleagues to test the Duke criteria. Using these criteria, the authors identified 67 of 158 consecutive patients with MIs as candidates for early discharge. Only 33 were actually discharged early (at 1 week), because most of the remaining patients did not have a home environment suitable for the planned intensive followup care. There were no deaths in either subgroup of patients at 6 months following discharge. There were five nonfatal complications at 6 months in the early subgroup and nine in the late.

This study and its accompanying editorial generated some lively correspondence (125,149). Many writers were concerned that the lack of randomly chosen control patients may have resulted in a control group that was different in subtle ways from the study group. Perhaps the less optimal home environments of the patients discharged late somehow contributed to their somewhat higher rate of nonfatal complications. Only a true RCT is capable of laying this kind of argument to rest.

Five RCTs have been reported that attempt to assess the health consequences of discharging low-risk MI patients early (5,17,71,86,118). Two are so methodologically flawed that their results cannot be interpreted with a satisfactory degree of reliability (5,71). The three remaining studies merit close scrutiny. In the earliest one, Hutter and his colleagues (86) compared 2 weeks of hospital care with 3 weeks in the treatment of patients with uncomplicated MIs. Their criteria for an uncomplicated MI were quite strict, and only 17 percent of patients assessed for possible inclusion in the study were actually included. At 6 months following discharge, 4 percent of the patients discharged at 2 weeks were dead compared with 7 percent of those discharged at 3 weeks.

The other two RCTs, one from Scotland (118) and one from Switzerland (17), used similar protocols and studied the difference between 3- and 4-week hospitalizations for patients with uncomplicated MIs. They excluded patients over 70 in addition to those with complicated MIs, but 69 and 80 percent of patients assessed were actually included in the study. In the Scottish study, 11 percent of patients discharged at 3 weeks had died by the end of the 12-month followup period as opposed to 15 percent of the 4-week group. In the Swiss study, 6 percent of the early group had died by the end of an 11-month followup period compared with 10 percent of the late group. None of the differences between the early and late patient groups in any of these three RCTs was statistically significant at the 5-percent level.

Constructing 95-percent confidence intervals for the difference in mortality between the early and late groups in each of these three studies is very informative. In the first study, the findings are compatible with differences ranging from 11 percent in favor of the early group to 5 percent in favor of the late group. In the second study, the data vary from 9 percent in favor of the early group to 2 percent in favor of the late group. And the third study varies from 13 percent in favor of the early group to 5 percent in favor of the late group. Because zero is included in all of these confidence intervals, the findings of these studies—that slightly fewer patients in the early group died—does not attain statistical significance.

The principal conclusion that one may draw from these studies is that early discharge of patients with uncomplicated MIs, as defined in the studies, is unlikely to pose a major health hazard. It may carry with it a significant benefit, about a 10-percent decrease in mortality. But it may also carry with it a small adverse outcome, about a 5-percent increase in mortality. The studies do not rule out the possibility of a negative impact on health. Studies of much larger sample size would be required in order to settle the question definitively. Finally, with respect to the Medicare program, it is important to recall that since two of the three rigorous RCTs excluded patients over 70, there is a special dearth of data from which to draw any informed conclusions concerning the elderly and early discharge for MI patients.

4.
Length of Stay and Outcome: Elective Surgery

4.
Length of Stay and Outcome: Elective Surgery

Surgeons now commonly encourage their patients to get out of bed and walk within the first day or two following many different surgical procedures. The history of attempts to achieve early postoperative ambulation is long and colorful, beginning in Chicago with Emil Ries in 1899 (143) and culminating with the widespread acceptance of the practice in the 1950's (8,18,19,23,28,36,59, 103,106,131,136,141). Once the principle of early ambulation was accepted, the next step was to experiment with shorter and shorter lengths of stay (LOS).

As in the myocardial infarction literature, three different kinds of studies have been performed evaluating the relationship between health outcome and LOS for elective surgery. The first group includes studies that have analyzed existing data on LOS, trying to explain differences. The second group comprises the large number of uncontrolled trials of early discharge following certain kinds of elective surgery. The third group includes a number of randomized clinical trials (RCTs) that have been carried out. Each of these groups of studies is reviewed in detail in appendix B. They are summarized here, focusing on RCTs.

The studies in the first group reported and analyzed LOS differences (64,65,75,92,119,157,168, 178). Some looked at differences between the United States and Great Britain, finding British lengths of stay longer, but not discovering why. The rest of the studies in this group reported large differences in lengths of stay across regions, among individual hospitals in the same region, and among individual surgeons. None found any relationship between LOS and outcome or quality of care, though these were usually crudely measured. None succeeded in explaining the LOS differences they observed.

The second group of studies represents the most common type of report found in this literature: the uncontrolled trial of early discharge (6,11,12, 30,33,34,41,45,49,55,84,87,122,150,152,153,169, 171,180). This usually takes the form of a single surgeon reporting his hospital's experience with a particular scheme of early ambulation and discharge. The specific surgical procedures most commonly studied are repair of inguinal hernia, varicose vein ligation, and hemorrhoid removal. The studies are almost exclusively British. Some report experience with same-day surgery, where the patient is treated in the same hospital by the same staff that might otherwise have used inpatient postoperative care. But in these studies patients are observed for a variable period following the procedure and then discharged home within 8 hours, without an overnight stay in hospital. Some of the studies report trials of short-stay surgery, where patients are simply discharged from the postoperative inpatient ward 1 to 3 days after surgery. Studies involving treatment provided at freestanding surgicenters have not been reviewed, because the scope of this case study is limited to LOS in the acute hospital.

The results of these studies are fairly consistent. Mortality is extremely small, less than 0.1 percent. Thus, it is difficult to evaluate these studies with respect to operative mortality. While a study would have to include over 1,000 patients before even one postoperative death could be expected to occur, each of these studies comprise at most a few hundred patients, frequently representing patients undergoing several different procedures. Yet, other outcome measures have been reported. Recurrence of inguinal hernia is perhaps the most important for evaluation of herniorrhaphy. The rate of recurrence is also usually low, often under 1 percent. It is also often difficult to determine the rate precisely from published reports, since patients have been followed over different postoperative periods, and total patient-years at risk for recurrence are usually not reported. In addition, postoperative complications have been reported as an outcome measure. These complications may range from small wound hematomas

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to serious infections. Their incidence is often not small, 10 percent in some of these studies. But it is usually difficult to interpret these figures, because different authors use different definitions of complications, and many authors do not state what these definitions are.

The most crucial defect in all of these studies, however, is the lack of control data. Without knowing precisely how a comparable group of patients fared when treated in the more traditional manner, one cannot evaluate these programs of early ambulation and discharge. Since the only outcome measure these studies are large enough to address is postoperative complications, the most important question that they leave unanswered is: Would the rate of complications have been even lower in a group of patients treated with longer periods of inpatient postoperative care?

The seven RCTs that have been performed in this area are potentially able to answer this question (3,4,48,54,121,151,154,161). Two of these studies are so methodologically poor that their results are impossible to interpret (48,54). One of them concerns LOS for gallbladder and ulcer surgery (161). This last study was very well done and demonstrated that LOS could be shortened by 2 days for these patients without any demonstrable harm. The remaining four RCTs tested the effects of early discharge, from 3 hours to 2 days following surgery, for patients undergoing inguinal herniorrhaphy, varicose vein ligation, and hemorrhoidectomy. The early discharge schemes were compared usually to traditional inpatient postoperative stays of 5 to 6 days. All of the studies were performed in Great Britain, and all excluded the elderly and patients with chronic disease. One excluded women.

The results of these four RCTs are remarkably consistent. There were no significant differences in any of them between the early and late discharge groups in operative mortality or hernia recurrence. No operative deaths were reported in any of the studies. This should not be surprising, however, since only 880 patients were involved in all four studies combined. All of the studies also reported higher postoperative complication rates for the groups of patients discharged early, from 8 to 15 percent higher than the patients with traditional lengths of stay. In two studies, the differences did not attain statistical significance; in one, the difference was of borderline significance (significant only at the 10-percent level); and in one, the difference was statistically significant at the 5-percent level. Finally, all four studies reported that their early discharge patients used significantly more outpatient services following their surgery than the patients discharged later. One of the studies tried to measure the extent of the savings realized by the early discharge (3,4). These authors reported a definite savings in hospital costs, but this was almost entirely offset by an increased cost in the early discharge group due principally to longer time off from work.

Interpreting the results of these studies raises some difficult questions. None of the studies was large enough to address the effects of early discharge for the two most clinically important outcomes of herniorrhaphy—operative mortality and hernia recurrence. This is particularly distressful, since herniorrhaphy is the prototype early discharge procedure. One is thus left in a situation similar to that found when examining the myocardial infarction literature. Very large and expensive studies would have to be done to answer the question of whether early discharge has a small, but clinically significant effect on these outcome measures.

Should such studies be done? Or should the risk be taken that operative mortality and recurrence rates may be somewhat higher in early discharge or outpatient surgery programs in order to reap their monetary benefits? And what are those benefits? The only RCT to address this issue explicitly found that men who had their hernias repaired on a short-stay basis were out of work somewhat longer than those patients who stayed longer in the hospital. Short-stay patients also consume somewhat more outpatient services than their longer staying counterparts. These findings suggest that any immediate savings realized from shorter hospital stays may be offset in part or in full by other costs. How the results of this RCT, which was performed in Great Britain, might have differed if done in the United States is difficult to assess.

Finally, these studies raise the difficult issue of how to balance the cost of a slightly higher postoperative complication rate against the benefit of

any monetary savings. All of the authors describe the complications they report as minor, but they do not provide any information on how long these complications persisted or how much disability they caused. Perhaps the higher rate of minor complications in the short-stay group accounted for their somewhat longer absence from work. Comparing these complications against the monetary savings is made all the more difficult, because so little is reported concerning them, and because the level of monetary savings is unknown. The studies reviewed here do not provide definitive answers to any of these difficult questions but have provided the data necessary to frame them.

5.
Length of Stay and Outcome: Obstetrics

5.
Length of Stay and Outcome: Obstetrics

Discussions of length of stay (LOS) in the obstetrical literature have centered around two different patient groups: patients with normal deliveries and patients with low birth weight infants. The discussion here will focus on U.S. studies of these problems. Although there has been some work done in Great Britain on normal deliveries (1,160,172), the long tradition of home births makes their experience very different from that of the United States and of only slight relevance. Some work has also been reported from developing countries (162,164), including a randomized clinical trial (RCT) from India (16). Major differences in maternal risk factors, prevalence of infectious diseases, and infant mortality rates between the United States and these areas make it impossible to generalize from this work to U.S. populations. As already discussed in other chapters, this case study does not deal with places of care outside the acute care hospital. Thus, a discussion of birth settings outside the hospital is beyond the scope of the present analysis. A review of the literature on the safety of different birth locations has recently been published (88).

Following World War II, rising birth rates rapidly led to shortages of maternity beds in U.S. hospitals, where the vast majority of birth takes place. This situation forced obstetrical departments to reduce lengths of stay for postpartum patients. One study appeared in 1962 that described this phenomenon. Hellman and Kohl (77) describe a study that compared outcomes among all patients discharged within 72 hours of a normal delivery with a random sample of all other maternity patients. They found no difference in the incidence of complications among either mothers or infants discharged early, as measured by subsequent development of illness in the infants when seen as outpatients, by readmission rates, and by mortality rates. More of the short-stay mothers were dissatisfied (7.7 percent) with their LOS than were those mothers who stayed longer (1.8 percent). The authors concluded that while they did not document any risks to early discharge, the risk of neonatal jaundice developing at home within the first week of life was significant and warranted home visits by a nurse during that time. The operation of such a program has also been reported (40).

This study has all of the problems of studies failing to employ random allocation procedures to select control groups. There is a clear potential bias in this study for the control group to be significantly sicker than the experimental group. The finding that on some measures the experimental group did slightly better than the control group is therefore not surprising. One does not know whether they did better simply because they were healthier or because they were discharged earlier. In fact, one may wonder about the possibility of early discharge having actually harmed the experimental group, since none of the differences in outcome was statistically significant. If there was a significant bias and the study group was healthier, perhaps early discharge canceled this advantage.

There is a single RCT involving early discharge of patients following normal deliveries. Yanover and colleagues (184) reported it in 1976 from San Francisco. This study compared discharge planned for 12 to 24 hours following delivery with discharge at 48 to 72 hours in a highly selected group of patients. Eligible patients were required to have had at most one other child; the mother was required to be between 19 to 35 years of age and of low medical risk; and the father was required to attend prenatal classes and to be living with the mother within 20 miles of the hospital. These criteria resulted in the elimination of 76 percent of the 362 mothers initially screened for participation in the study. The remaining 88 patients were randomly assigned to study and control groups. Study patients were discharged at 12 or 24 hours, providing the mother and infant met certain criteria designed to identify fitness for early discharge. These included the absence of fever, the presence of normal blood pressure, and the absence of excessive vaginal bleeding in the mother. The criteria also included a birth weight between 6 and 9 pounds, normal vital signs, absence of

feeding difficulty, and an Apgar score of eight or greater at 1 minute for the baby. Following discharge, the study group received home visits from a specially trained perinatal nurse practitioner. Failure to meet discharge criteria prevented 23 of 44 study patients from going home within the targeted period of 24 hours after delivery. The average LOS was considerably lower in the study group (1.8 v. 3.4 days), and none of the control patients went home within the first 24 hours.

The patients were followed for 6 weeks after discharge. No statistically significant differences were measured in rates of complications among infants or mothers, although the rate of morbidity among infants in the study group was less than among those in the control group (9 v. 20 percent). None of the mothers was readmitted during the 6-week followup period. The authors concluded that their program was safe and stated that they hoped that it would promote better bonding between mother and infant, although they did not attempt to measure this phenomenon. They also estimated that the costs of the program were about the same as the savings that resulted from early discharge.

This study documented that, for a highly selected group of patients, the combination of early discharge, prenatal education, and a program of home care produced results comparable to more traditional care. Once again, small sample sizes prevent one from drawing any solid conclusions about the effect of the program on infrequently occurring events such as neonatal mortality. The study almost demonstrated a statistically significant benefit of the program in reducing neonatal morbidity. The 95-percent confidence interval for the difference in morbidity rates for the infants in the study and control groups ranges from 26 percent in favor of the study group to 3 percent in favor of the control group. From a clinical viewpoint, one must believe that the program of home followup care included in this study was heavily responsible for this result, with some contribution from the decreased exposure of the infant to the hospital environment by early discharge. It is not at all clear that the same results could be achieved by a program that involved only early discharge and did not also provide home care. From an economic viewpoint, therefore, this study does not offer great hope of saving large amounts of resources by drastically reducing LOS for the millions of patients discharged annually with normal deliveries. Moreover, this study does not provide evidence that such a policy would be safe.

LOS for uncomplicated deliveries in the United States has fallen steadily since 1968, even in the absence of pressure from the postwar baby boom. Table 12 shows how each region has declined in this measure. All but the South experienced declines greater than the decline in average LOS for all patients in their regions. Based on 1978 regional LOS patterns, if all regions were able to achieve the same 1.8 day average LOS for 24 percent of their patients with uncomplicated deliveries as this study did, then a total of about 848,000 postpartum hospital days could have been saved: 11 percent of the total spent. While this calculation clearly shows the great potential saving in this area, one must hesitate from generalizing too widely from a single study. One must be even more careful to avoid generalizing beyond the limits imposed by the study itself. This was a study of early discharge, education, and home followup care, where the costs of the second entirely canceled the savings from the first. Additional research is required before it can be con-

Table 12.—Regional Trends in Length of Stay for Uncomplicated Delivery

Region	Length of stay (days)		Percent change
	1968	1978	1968-78
Northeast	4.7	3.9	-17
Northcentral	4.4	3.7	-16
South	3.5	3.1	-11
West	3.4	2.5	-26
United States average	4.1	3.3	-20

SOURCE: *Vital and Health Statistics*, series 13, No. 64, DHHS publication No. (PHS) 82-1725 (Washington, D.C.: National Center for Health Statistics, 1982).

cluded that early discharge alone is safe and economical.

Low birth weight infants are generally defined as those born weighing 5 pounds or less at birth. Traditionally, in the United States, they have been kept in the hospital until they have attained a weight of 5 to 5½ pounds. This procedure was questioned in the late 1960's and early 1970's by three studies that allowed these infants to go home prior to the attainment of fixed target weights. Instead, they used criteria designed to assess the infant's ability to function satisfactorily at home. The first study (14) examined a group of 68 babies who went home with a mean discharge weight of 4½ pounds after an average LOS of 11 days. This group was compared to a sample of other low birth weight infants cared for at other hospitals in the same State. These infants were discharged at an average weight of 5⅓ pounds after an average LOS of 22 days. No significant illness occurred in the study group. The two other early studies (9,13) were uncontrolled. Both found a low incidence of problems following early discharge for a group of low birth weight babies. Bauer and Tinklepaugh (9) reported that 2 of 57 such infants did poorly after discharge, one recovering after a period of slow growth, the other succumbing to sepsis that apparently began 5 days after discharge. Berg and Salisbury (13) extended their previous series and reported no fatalities at 2 months after discharge in a group of 170 early discharges. However, they found that one infant developed pneumonia and one pair of twins experienced poor weight gain. Without randomly selected com parison groups, these data are difficult to interpret.

One RCT has been done in a U.S. population in this area. Dillard and Korones (42) reported a study in which low birth weight infants in Memphis were randomly assigned to study and control groups. In the study group, infants were required to attain a weight of 2,000 gm (4 pounds 6 ounces) prior to discharge; control infants were required to weigh the usual 2,268 gm (5 pounds). Other criteria were included to ensure that the babies were healthy and gaining weight consistently before discharge. Of 548 infants randomly assigned, 51 died prior to discharge, and another 87 were excluded because their discharge weights were more than 100 gm over the target for their group. Average LOS was 19 days for the study group and 25 days for the controls. There was no difference in average daily weight gain as outpatients. At four weeks, 4 percent of the study group and 5 percent of the controls had been rehospitalized, while 0.5 percent of each group had died. A similar study from England (39) showed no readmissions in three months for 20 early discharge low birth weight infants and 20 controls.

The study reported by Dillard and Korones (42) was a well-executed RCT. The principal problem in interpreting the results of the study is the familiar one of statistical power. The study had a very small chance of detecting any clinically important differences in mortality rate. Assuming the mortality rate in the control group to be 0.5 percent, as measured, the study had only a 14-percent chance of rejecting the null hypothesis of no difference even if the study group's mortality rate had actually been twice that. As with elective surgery, the sample size in this RCT was inadequate to measure important differences in mortality.

On the other hand, the study was not so bad with respect to readmission rates. The 95-percent confidence interval for the true difference between the study and control groups in hospital readmission rates ranged from 4.9 percent in favor of the study group to 3.6 percent in favor of the control group. If the true readmission rate for the study group had been twice that actually measured for the control, the sample sizes in this study would have given it a 58-percent chance of detecting this difference at the 5-percent level of significance. The study did show that this particular early discharge program did not result in large differences in either positive or negative events for the study group. Whether patients discharged early were exposed to a somewhat greater risk of dying or a slightly increased risk of readmission to hospital has not been proven.

6.
Length of Stay and Outcome: Psychiatry

6.
Length of Stay and Outcome: Psychiatry

The trend toward less inpatient hospital treatment for psychiatric patients has been established for well over a decade. A series of randomized clinical trials (RCTs) appeared in the 1970's that compared various regimens of brief hospital treatment with more traditional, longer periods of inpatient care for serious psychiatric disorders.

Herz and colleagues (80) reported the first large RCT studying the appropriate length of stay (LOS) for psychiatric patients, 49 percent of whom were schizophrenic. All patients newly admitted to the psychiatric unit of Columbia Presbyterian Medical Center in New York were evaluated for possible inclusion in the study. Seventy-nine percent of all patients screened were rejected—patients who were too ill or too healthy, those with uncooperative families, and those with concomitant physical illness. The remaining 90 patients were randomly assigned either to a control group receiving usual 24-hour per day inpatient treatment or to a study group that was treated with day care in the same ward, 8 hours per day, 5 days per week. LOS for the initial hospitalization was drastically reduced—48 days for the study group as opposed to 139 days for the control. During the followup period, the control group was rehospitalized more often and demonstrated more psychopathology than their counterparts in the study group.

Caffey (29) reported a trial that included two study groups in addition to a control group treated with the usual inpatient care. One study group received a maximum of 21 days hospitalization followed by intensive outpatient treatment. The second study group received the usual hospital care followed by the same outpatient treatment. The study accepted 201 schizophrenic men after a larger, but unreported, number were screened at the 14 participating Veterans Administration hospitals using criteria similar to the first study. The study was successful in discharging the short hospitalization group, as 81 percent were released within a month. In contrast, only 33 percent of the control group and 24 percent of the second study group were discharged during the first month. Readmission rates during the year after discharge were equal in the first study and control groups (34 percent) and somewhat lower in the second study group (24 percent). The average length of time spent out of the hospital prior to readmission was 20 days longer in both of the study groups compared to the controls. There was no difference among the groups in measured levels of psychopathology or functioning abilities.

Glick and his coworkers (56) randomly assigned consecutively admitted patients to short- or long-stay groups. They analyzed their results separately for their nonschizophrenic and their schizophrenic populations. The 74 nonschizophrenics were evenly divided among study and control groups. The study patients averaged only 26 days during their initial hospitalization compared with the control group's 100 days. At 1 year following discharge (58), the long-stay patients had experienced twice as many readmissions (0.4 readmissions per patient for the long-stay group v. 0.2 per patient for the short-stay group) and almost twice as long a LOS per readmission (35 v. 19 days). In addition, fewer patients avoided rehospitalization in the long-stay group (76 v. 84 percent). None of these differences is statistically significant at the 5-percent level. Functional evaluation showed that on the vast majority of measures, no differences could be found between the two groups; on 2 of 27 measures the long-stay group showed a slight advantage. These differences were considered clinically insignificant as the authors concluded that their study did not provide "strong support for the use of the more expensive longer hospitalization for nonschizophrenic patients."

The results for schizophrenic patients showed similar evidence of increased use of psychiatric services for the long-stay group (57,68). At 2 years following discharge, the long-stay group had spent almost twice as many days in the hospital as the short-stay group (17 days per patient v. 9 days). They also averaged 46 percent more psychotherapy visits per month. In addition, fewer patients

in the short-stay group were taking phenothiazines, and those who were taking the drugs were receiving a lower dose than their counterparts in the long-stay group. While the hospitalization results were not statistically significant, the drug results were. Functional testing showed that at 1 year after discharge, the long-stay group scored slightly better on global measures of severity of illness than the short-stay patients. These differences, which were statistically significant, had decreased by the 2-year followup assessment. The authors noted that the better results might have been caused not by the longer initial hospital stay directly, but rather by the greater exposure of long-stay patients to psychiatric care during the followup period. In addition to this possibility, this study has been criticized for inadequate comparability of study and control groups despite random assignment. It has also been criticized for inappropriate treatment of the short-stay group, in particular the failure to provide post-discharge treatment (81).

Herz, et al. (78,79), have also reported a study in which newly admitted patients, selected because they had cooperative families, were randomly assigned either to one study group in which a brief planned hospitalization was followed by day care, to a second study group in which brief hospitalization was followed by discharge to the community, or to a control group treated with the more lengthy, traditional period of hospitalization. A total of 175 patients were randomly allocated to these three treatments. Of the total, 63 percent of the patients were schizophrenic. It is not clear how many were rejected by failing to meet the selection criteria. LOS for both study groups averaged 11 days; the control patients stayed an average of 60 days. All groups improved in their measured levels of psychopathology; there were no statistically significant differences in improvement among groups. Furthermore, there were no differences among groups in readmission rates. However, both study groups spent far fewer total days in the hospital during the 2-year followup period than the control patients. The study group with home care spent an average of 27 days per patient in the hospital during the followup period, the study group without home care experienced 47 days per patient, and the controls experienced 115 days.

A similar study was performed by Hirsch and colleagues (83,100) in London. Selection criteria similar to those noted above were used to identify 224 candidates for this RCT, in which the study patients were treated with a brief period of hospitalization and the controls with the usual, longer period. The study patients spent an average of 22 days in the hospital, exactly twice as long as the study patients in the previous study, and the controls averaged 28 days. No differences in readmission rates or days hospitalized during the followup period were found. Nor were any differences measured between the two groups in improvements in psychopathology; 81 percent of the study group improved as did 79 percent of the controls.

Kennedy and Hird (95) reported a study in which 76 percent of newly admitted patients were randomly assigned either to one brief treatment ward or to two traditional treatment wards all at the Royal Edinburgh Hospital. The only reported criterion for rejection from the study was continuity of care—i.e., if a physician felt that particular patients would be best served by remaining with staff who had treated them previously, they were withdrawn from the study. This criterion applied to 27 percent of patients randomly assigned to the experimental ward and to 22 percent of those assigned to the control wards. There were 86 study patients and 161 controls. Significant differences were recorded between study and control patients in average LOS for the initial hospitalization (11 v. 24 days) and in average hospital days during the entire study period (17 v. 31 days). It should be noted that, as with the previous study, there was no difference between the groups in followup hospital days. Thus, the difference in total days was entirely accounted for by differences in the initial LOS. No differences in outcome measures assessing the patients' psychopathology by interviews with patients and families were found. Nor were there any differences in the degree to which patients burdened their families. Control patients were seen more often by their general practitioners; they averaged one contact per patient in the 3 weeks after discharge while controls averaged 0.5.

Rosen and colleagues (114,115,148) have published a series of papers describing a study that

was not strictly a RCT. The study is reviewed here briefly, because it is cited prominently in the field as an influential work. Patients were allocated to two experimental wards and three control wards on the basis of bed availability. No clinical or sociodemographic characteristics were used to assign patients. For reasons not explained, control beds were filled first, then study beds. The study evaluated results only for the patients officially discharged; patients leaving against medical advice were excluded. The proportion of patients in the study and control groups who chose this option was similar (28 v. 27 percent). Short-stay patients stayed an average of 86 days; they had a target stay of less than 90 days. Long-stay patients experienced an average LOS of 179 days; they were discharged when their attending physicians felt that maximum benefit had been achieved. The fact that the study was conducted from 1970-71 may explain why the short-stay group's LOS was so much longer than that employed in later studies. Despite lack of random assignment, study and control groups were similar in most demographic and clinical measurements. The short-stay group did exhibit more cognitive disturbance than the long-stay group; this was the only significant initial difference.

The study also found that the short-stay patients had improved slightly more than the long-stay patients at the time of their respective discharges. At the end of the 3-year followup period, 24 percent of the 58 control patients had been readmitted compared to 22 percent of the study patients. The control patients were readmitted almost twice as frequently (2.1 v. 1.1 readmissions per patient) and for longer periods each time (4.6 v. 2.9 months) than the study patients. Followup at 3 and 4 years showed that the two groups were similar in exhibiting mild to moderate psychopathology, but the long-stay group did score slightly better where the differences were statistically significant. None of these differences, which were few in number, was felt to be clinically significant. The results of this study, while consistent with those of the main body of RCTs discussed above, can be criticized, because a true random allocation procedure was not used. While it is difficult to determine precisely how the assignment scheme actually used might have biased the experiment without a great deal more information, there is no question that the risk of bias was greater in this study than in a true RCT.

The information generated by these studies is largely internally consistent. In four of seven studies, the long-stay groups experienced more days in the hospital during the followup period than did the short-stay groups. In five of seven studies, no clinically significant functional differences could be found between the two groups. One study found slight differences in favor of the short-stay group, and one study found similar differences in favor of the long-stay group. While the authors of these studies discussed Type II errors no more frequently than the authors of the other studies reviewed here, it is evident that the power of these studies was greater than those previously discussed. Because data on standard deviations of functional assessment measures were not given, one cannot calculate precise estimates of power from the data reported in these studies. However, several of them noted statistically significant improvements over time in their patient populations, frequently about 20 percent (95). One infers that similar differences could have been detected had they been present between study and control groups.

The general conclusion that emerges from this review of the psychiatric literature is that prolonged initial hospitalization for acutely ill psychiatric patients is associated with greater levels of treatment following the initial period and shows no better results than a period of briefer initial hospital treatment. There is thus good evidence that short stays are not harmful and should be employed where possible. This same conclusion was reached by a prominent psychiatrist in a recent review (96). At the same time, it is also clear from the widely divergent treatment patterns and heterogeneous patient populations employed in this group of RCTs that the optimal LOS for specific subgroups of psychiatric patients has yet to be established.

7.
Summary and Implications for Research and Policy

7. Summary and Implications for Research and Policy

SUMMARY

This case study has demonstrated that large differences exist in hospital length of stay (LOS) among geographic regions in the United States. The Northeast region has consistently exhibited the longest lengths of stay in the Nation and the West the shortest. Since 1968, despite a trend toward shorter LOS across all regions, the differences between East and West have persisted at about the same level. Although no attempt was made here to place a dollar value on these differences, there can be no doubt that the magnitude of the variation in LOS is potentially of considerable economic significance. The evidence is persuasive that these differences are not a function of differences in population characteristics such as age, sex, or race.

The extent to which case mix or severity of illness differences can account for LOS variations is more controversial. Adjusting regional LOS for differences in the distribution of diagnostic groups at a general level of aggregation reveals little effect on regional differences in LOS. However, severity of illness could explain some of the observed variation in regional LOS. This could occur if physicians in the Northeast systematically admit to the hospital only the sickest patients in each diagnostic category while those in the West admit the least sick patients.

Case mix differences among regions have rarely been studied at this level of clinical detail. In the Professional Standards Review Organization (PSRO) studies discussed in chapter 2, five conditions were studied in such a way as to be able to draw inferences concerning differences in severity of illness. In the Utah/Central Massachusetts studies there was a small, but statistically significant difference in severity of illness of cholecystectomy patients, with the eastern PSRO experiencing the more severe case mix. There was no difference in case mix for myocardial infarction (MI) patients. In the Baltimore/Portland studies, there were significant differences in case mix for MI and congestive heart failure (CHF) patients but not for angina patients. In both cases where the distribution of cases was different, the eastern PSRO experienced the more complex case mix.

In two of the three examples of differing case mix, it was possible to adjust average LOS for these differences. Table 13 presents these results.

Table 13.—Case Mix Differences Between Baltimore and Portland

Condition and severity class	Baltimore		Portland	
	Number of cases	LOS (days)	Number of cases	LOS (days)
1. Uncomplicated MI	21	13.6	11	8.0
2. Moderately complicated MI	88	16.9	63	11.4
3. Severely complicated MI	26	20.5	4	9.3
Total	135	17.1	78	10.8
Adjusted MI LOS		(16.9)		(10.6)
1. Uncomplicated CHF	15	9.1	13	4.7
2. Moderately complicated CHF	27	11.3	34	8.3
3. Severely complicated CHF	71	14.0	19	7.0
Total	113	12.7	66	7.2
Adjusted CHF LOS		(12.3)		(7.1)

Calculated from data in A. Ankrum, Baltimore City Professional Standards Review Organization, personal communication, November 1982.

The adjustments were performed by using the entire study population of both PSROs as the reference population and then by applying each PSRO's severity-class-specific LOS to the reference population. Thus, adjusting for case mix differences failed to affect the difference between PSROs in average LOS for MI patients and reduced the difference for CHF patients by only 5 percent. This might be explained by the fact that while LOS was related directly to severity of illness in the Baltimore patient population, the Portland patients did not show the same simple relationship. In Portland, the most severe patient group in both cases had a shorter LOS than the group with moderate severity. This, in turn, may have occurred because there were more early in-hospital deaths in the Portland group of most severely ill patients. These data are not contained in the reports from which this information is drawn. This apparent discrepancy may also be due to a failure to adequately define severity of illness.

Whatever the reason for the short LOS in the Portland patient populations, it is apparent that adjusting for case mix differences in these two instances does not diminish differences in average LOS. On the other hand, it is also true that for these two conditions and for each of the other three studied, LOS was shorter in the western PSRO in each severity class. In reviewing all of the evidence, therefore, while allowing the possibility that some case mix differences between East and West may exist, one must conclude that such differences are most unlikely to account for a large part of observed regional LOS variations.

If substantial differences in LOS remain after controlling for case mix, one is forced to conclude that physicians must employ different treatments for the same conditions in the East and West. Research in this crucial area simply does not exist. The Utah/Central Massachusetts PSRO study provided some intriguing data on differing practices with respect to ambulation and feeding of postoperative cholecystectomy patients. Except for this example, there has been no attempt to gather this kind of information at a detailed clinical level. Furthermore, no studies have adequately addressed the question of whether these different lengths of stay have any impact on health status. There is simply no evidence as to whether western patients fare better or worse with their short lengths of stay compared to eastern patients.

In order to address the critical question of how hospital LOS is related to health outcome, the medical literature has been reviewed to discover the extent to which good quality research had established what LOS produces the best health outcome in specific clinical conditions. The goal of this analysis was to ascertain whether LOS standards could be inferred from these research studies and used to assess the appropriateness of regional LOS differences. If a medically optimal LOS could be determined from analyzing the medical literature, then regional LOS patterns could be compared to this standard, evaluating which region's LOS is too high and which is too low.

The medical literature has been examined, focusing on studies in which researchers attempted to change LOS for specific medical conditions in order to improve health outcomes. Randomized clinical trials (RCTs) have been given special attention in this review of the medical literature, because their design is most likely to produce valid results. Study and control groups are as comparable as possible in order to be as certain as possible that observed differences in outcomes are attributable to the experimental treatment. The RCT is certainly not a guarantee of such results and generalizability may be limited. Nor is the RCT the only informative design for medical research. Nevertheless, it is the most effective approach to the complex questions addressed here.

At least one methodologically sound RCT was found in five different clinical areas: MI, elective surgery (primarily inguinal herniorrhaphy), low-risk obstetrics, low birth weight infants, and psychiatry. All of these RCTs experimented with shorter lengths of stay than had been traditionally used, none with longer. All of the studies concluded that the shorter LOS was not harmful. Table 14 summarizes the overall characteristics and results of these 17 RCTs. In reviewing these studies, it is noteworthy that all but one study in each of the MI and surgery categories excluded the elderly from participation. Therefore, even the limited conclusions one can draw from these studies do not apply to the elderly. In addition,

virtually all of these studies excluded a significant proportion of patients screened for potential participation, the single exception being the study on low birth weight infants from Memphis. In all cases, patients were excluded because they were felt to be too sick to be candidates for early discharge.

Only two studies discussed mortality as an outcome: MI and low birth weight infants. The trend in the MI studies was for the groups with the shorter LOS to have slightly lower mortality, but these differences did not achieve statistical significance at the 5-percent level. There were no differences between groups in the low birth weight infant studies; the two studies combined reported only a single death in each patient group.

In table 14, morbidity is defined as follows: for MI patients, the rate of nonfatal cardiovascular complications during the followup period; for elective surgery, the rate of postoperative complications; for low-risk obstetrics, the rate of neonatal complications; and for low birth weight infants and psychiatry, the rate of readmission during the followup period. Only one surgery study and three psychiatry studies reported that their differences in morbidity were significant at the 5-percent level. However, the authors of the surgical studies stated uniformly that the complications observed were of little clinical significance and should not be considered reason enough to discontinue the practice of early ambulation and discharge. It should also be noted that some of these studies presented data on morbidity other than those summarized in table 14. These morbidity data were discussed in relation to each study and have been omitted for simplicity. They do not alter the general conclusions of this discussion.

The authors of all of these studies concluded that the experimental short LOS could be employed with safety, because there was no statistically significant increase in morbidity or mortality. As previously discussed, only in the case of psychiatry does this conclusion seem justified by the data. For the other areas, the most distressing problem is the lack of statistical power to detect clinically significant increases in morbidity or mortality. None of these studies had less than a 25-percent chance of making a Type II error in accepting the null hypothesis of no difference if in fact a clinically significant difference existed. Thus, the statistical power in these studies was always less than 75 percent. Increasing sample sizes would increase the power (and decrease the chance of a Type II error), but in clinical trials, sample size is often kept small in case of harmful effects to patients.

The problem in interpreting negative clinical trials has been reviewed by Freiman and colleagues (52). They were concerned about the possibility that a new treatment might be abandoned

Table 14.—Summary of RCTs

	MI	Elective surgery	Low-risk obstetrics	Low birth weight infants	Psychiatry
1. Number of methodologically sound RCTs	3	5	1	2	6
2. Number of studies excluding the elderly	2	4	NA[a]	NA[a]	0
3. Average percent of screened patients accepted into study	55	68	24	100	68
4. Mortality: number of studies where					
a. E > L[b]	0	nd[c]	nd	0	nd
b. L > E	3	nd	nd	0	nd
c. L = E	0	nd	nd	2	nd
5. Power: number of studies with power ≥ 0.75 to detect 50% increase in mortality	0	0	0	0	nd
6. Morbidity: number of studies where					
a. E > L[b]	1	4	0	0	0
b. L > E	1	1	1	1	5
c. L = E	1	0	0	1	1
7. Power: number of studies with power ≥ 0.75 to detect 50% increase in morbidity	0	0	0	0	nd

[a]NA = not applicable.
[b]E = early discharge group; L = late discharge.
[c]nd = data not reported or cannot be derived.

after a negative clinical trial in which no difference was observed between study and control groups. In that circumstance, a Type II error might lead to a failure to appreciate a beneficial effect of a new treatment simply because the sample size was too small to demonstrate a statistically significant difference.

The problem in the present analysis is just the reverse—a concern with missing a harmful effect of early discharge. A negative RCT in this situation might mistakenly conclude that early discharge was safe, when in fact, the study could not detect a clinically significant harmful effect due to small sample size. All of the RCTs except the psychiatric studies have some degree of this problem in evaluating differences in mortality. In elective herniorrhaphy, low-risk obstetrics, and even low birth weight infants, the rates of reported mortality are so low that very large sample sizes would be required in order to have any reasonable chance of observing even large differences among different treatments. The sample sizes required for MI studies are somewhat less, but still greater than those employed in the reported RCTs. The same arguments apply to the morbidity measures reported.

In conclusion then, one cannot exclude the possibility that patients with uncomplicated MIs, elective surgery, uncomplicated deliveries, or low birth weight infants may experience a clinically significant increased risk of mortality or morbidity when discharged earlier than more conventional treatment practices. The RCTs reviewed do establish persuasively the lack of extremely large negative or positive effects on health outcomes of early discharge in these clinical areas. Studies with larger sample sizes will be required to evaluate the possibility of small to moderate effects.

In addition to failing to establish unequivocally the safety of early discharge, the medical literature also fails to shed additional light on the meaning of regional LOS variations. It cannot be inferred that western lengths of stay are as safe as eastern, because a clinically significant adverse effect of early discharge cannot be ruled out in those clinical areas studied. Further, the precise ways in which eastern and western physician practices differ are unknown. Since these practices have not been explicitly compared in any of the reviewed RCTs, it is also unknown whether the results of the RCTs would differ between regions. How long are MI patients kept at bed rest in the East and the West? What kind of anesthesia is used in herniorraphy patients in the East and the West, and when are they first ambulated? When are low birth weight infants sent home in the East and the West and are the discharge criteria (implicit or explicit) at all similar to those used in the RCTs summarized here?

Moreover, only a limited number of clinical areas have been studied. The LOS differences between the East and the West are pervasive—found in virtually every diagnostic group. As demonstrated, the medical issues involved in the relative safety of early discharge vary enormously from one clinical condition to another: from whether a 4½ pound neonate is feeding adequately to whether a 60-year-old man with an uncomplicated MI should be allowed out of bed on the sixth or seventh day of his hospital stay. This heterogeneity precludes generalizing the results of a few RCTs in a few clinical areas to other patients with other conditions. No information exists at all that would allow conclusions to be drawn concerning the relative safety of various lengths of stay in these unstudied clinical areas, which comprise the vast majority of hospital patients.

A recent review by Berk and Chalmers (15) evaluated data in the literature for evidence that outpatient care could be safely and economically substituted for inpatient care. They reviewed some of the same RCTs on early discharge that were reviewed in this case study. They concluded that many of the studies were methodologically flawed and that little support was available for the proposition they set out to investigate. Although the analysis in this case study did not address the issue of savings attributable to short LOS, a similar conclusion has been reached with respect to the safety of short lengths of stay.

IMPLICATIONS FOR RESEARCH

Two overall suggestions for future research emerge from this study. The potential economic significance of regional LOS variations combined with a general lack of understanding of their health implications make a large-scale study of current medical practice very important. A diverse sample of clinical conditions should be selected for which large differences in regional LOS exist and for which a significant proportion of the inpatient population is affected. A protocol should be designed to sample patients in several eastern and western localities. Thus, severity of illness can be precisely measured, regional differences in physician practices can be recorded within severity classes, and regional differences in outcomes can be assessed. Only a study of this nature can remove the mystery concerning the meaning of regional LOS variations.

The second research suggestion concerns future RCTs designed to test the efficacy of early discharge. The central problem is that very large RCTs will be required to address definitively many of the remaining questions concerning the safety of early discharge. At the same time, some of the risks that such studies would be designed to assess are quite small. The more infrequent the event, the larger the sample size, and the more expensive the study needed to detect changes in its incidence. A study is needed to set research priorities in this area. In which clinical areas are additional data on the safety of early discharge most critically needed? This question should be evaluated both from the perspective of the magnitude of the risk involved and the potential size of the economic benefit to be obtained from shortening LOS. Such a study should survey all clinical conditions for which substantial regional differences in LOS exist and should not be limited to a consideration of those areas in which studies have already been done.

IMPLICATIONS FOR POLICY

What are the policy implications of this review? Should western lengths of stay serve as standards and somehow be enforced on the rest of the country? As noted in this case study, with the exception of psychiatric hospitalization, medical research has thus far failed to exclude the possibility that early discharge is harmful. Only a handful of clinical conditions have been studied, frequently excluding substantial segments of the population. The most common finding of these studies is that for the outcome measures employed, there is no statistical difference between early and late discharge groups. Faced with this lack of definitive data, what conclusions may be drawn? The answer depends somewhat on where one considers the burden of proof to lie. Must proponents of early discharge prove that it is beneficial, or at least harmless? Or must proponents of longer hospital stays prove that early discharge is harmful? Physicians are likely to adopt the first position, while policymakers may prefer the second.

This question is made all the more difficult to answer, because the economic benefit of early discharge programs is difficult to specify. If the economic benefit of shortening LOS were indisputably large, then the case for early discharge would be much stronger. As already discussed, however, it is far from clear how reducing LOS produces monetary savings, either to society or to government health care programs. At one extreme, LOS may be shortened by uniformly eliminating days at the end of hospital stays through early discharges. If, however, these patients are replaced by patients who require more services per day, the net effect of such a program of LOS reduction would be to increase total costs.

On the other hand, if LOS is reduced to the point where an individual hospital experiences a large decrease in its occupancy rate and if this deficit is not replaced, the hospital may close entirely or convert some portion of its beds to another,

less costly use. In this circumstance, reducing LOS could result in a net reduction in total costs. Thus, whether LOS reductions actually save money will depend heavily on precisely how they are brought about.

How to reduce LOS so that such savings occur is an area totally unapproached by current health services research. No U.S. study has even attempted to address this question. The one British study that did address the issue (3,4) looked only at the effect of early discharge on the costs of care for the patients in the study. No attempt was made to assess the effect of early discharge on total health care costs to the community. Therefore, in the face of limited data, uncertain benefits, and possible harm, the case for early discharge seems unconvincing at best.

Does this conclusion change if one considers the perspective of the Medicare and Medicaid programs? Many of the most rigorously designed studies in the medical literature that tested the effects of early discharge programs excluded the elderly from their study populations. No study has been performed that specifically and rigorously examined the effect of early discharge on a Medicare or Medicaid population. These populations are sufficiently different from the general populations ordinarily represented in the medical literature that one must be hesitant before extrapolating results from the one to the other. Thus, the data available to judge the effects on health outcomes of early discharge for the poor and the elderly are even more scarce than for the rest of society. It is therefore even more difficult to make a strong argument in favor of early discharge in the context of the Federal health programs.

Hospital LOS varies greatly from place to place across the United States. This review has demonstrated that very little is known concerning the health consequences of these variations. Only future research along the lines described earlier in this chapter can provide the basis for rational judgments about whether hospital stays are too long in the East, too short in the West, both, or neither. Existing data cannot exclude any of these possible interpretations.

Appendixes

Appendix A.—Length of Stay and Outcome: Myocardial Infarction

Myocardial infarction (MI) is the clinical condition that has received the most attention from researchers with respect to how much hospital care is required for treatment. Prolonged bed rest was the hallmark of the treatment of acute MI patients until the 1960's. Lewis (109), for example, recommended 8 weeks of bed rest in 1937. White (179) recommended 1 month of bed rest in 1945. These recommendations were based in part on fears that inadequate bed rest would lead to cardiac rupture or ventricular aneurysm formation (112,123). A few dissenting voices were heard (70,43). In 1947, Asher (8) wrote colorfully of the dangers of too much time spent in bed:

> Look at a patient lying long in bed. What a pathetic picture he makes! The blood clotting in his veins, the lime draining from his bones, the scybala stacking up in his colon, the flesh rotting from his seat, the urine leaking from his distended bladder, and the spirit evaporating from his soul.

He thought the traditional 6 weeks of bed rest following a MI was unsupported by good evidence of efficacy. Levine and others (10,107,108) recommended that MI patients be placed in a sitting position as soon as possible, believing that greater rest was afforded to the heart in this position.

In 1950, Irvin and Burgess (90) discussed the disadvantages of bed rest, including poor circulation to the basilar part of the lungs, worsened congestive failure, increased thrombophlebitis and pulmonary embolism, negative nitrogen balance, and negative psychologic sequellae. They recommended a period of 2 weeks bed rest and 4 weeks hospitalization for MI patients. Brummer and his colleagues (27) reported on 258 consecutive MI patients whom they treated with an average of 16 days bed rest and 23 days hospitalization. They reported two cases of sudden death during the ambulation period in the hospital, both occurring in patients who had been kept in bed longer than usual, presumably to treat complications. They also noted one patient with sudden death and 21 with recurrent MI during the first month after discharge. Although this incidence of recurrent infarction seemed to the authors to be higher than they expected, they concluded that on the whole early ambulation should be prescribed for MI patients.

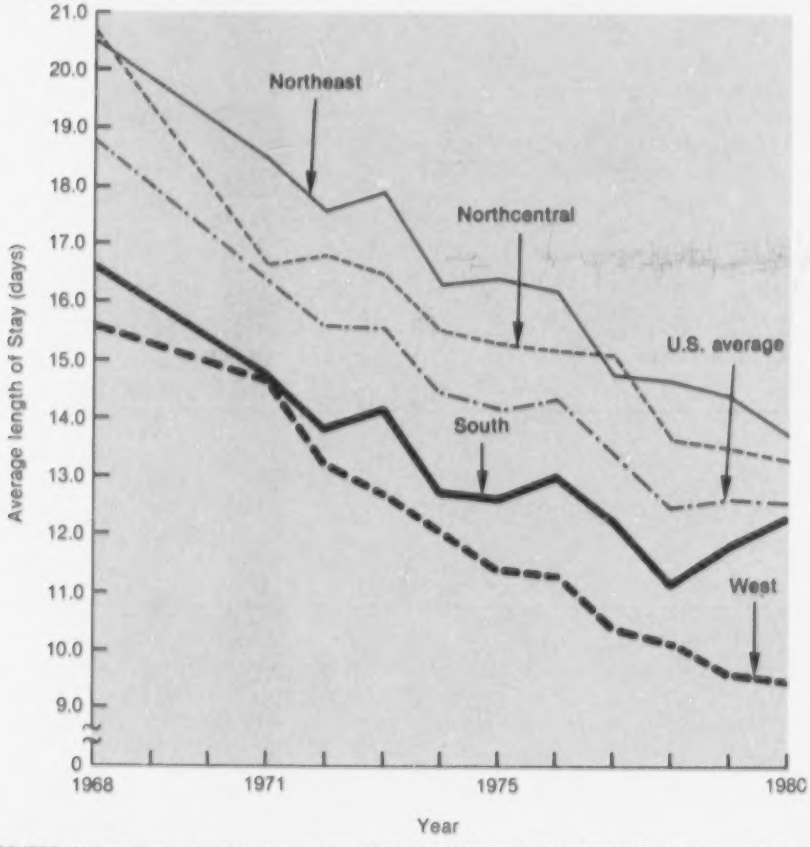
Beginning in the 1960's and accelerating into the 1970's, increasing numbers of research studies of early ambulation for MI patients were published. Associated with the appearance of these studies has been a rapid decrease in the U.S. length of stay (LOS) for MI patients. Figure A-1 describes the extent of this decline by region since 1968. From 1968 to 1980, LOS for MI patients in the United States has declined 33 percent, as opposed to 14 percent for all patients. The decreases for each region have been striking, with the West declining the most (38 percent), followed by the North-central (35 percent), the Northeast (32 percent), and the South (26 percent).

Three fundamentally different types of studies have been reported in the literature. The first group comprises studies that analyzed clinical data trying to explain variations in treatment practices or to identify characteristics of low-risk MI patients who might be candidates for early discharge. Studies in the second group reported on the effects of early ambulation and discharge programs for MI patients without providing any control data. Studies in the third group also reported on early ambulation and discharge but included control data for comparison, in some instances from randomized clinical trials (RCTs). The studies in each of these groups are reviewed in turn.

Two retrospective studies documented large variations among individual physicians in the care of MI patients. Heasman and Carstairs (76) found that in 1967 among Scottish physicians who each cared for at least 20 MI patients, the average LOS per physician ranged from 10 to 36 days, with physicians at teaching hospitals experiencing a lower median LOS (20 days) than physicians at nonteaching hospitals (25 days). No attempt to adjust case mix was made. Duke (46) examined 313 MI patients at a single hospital in Connecticut, all under 65 years of age from 1965 to 1968. He found that physicians differed in how much bed rest they prescribed (the average physician varying from 7 to 15 days), in how long their patients stayed in the hospital (from 21 to 29 days), and in how much bed rest they prescribed as a proportion of the total hospital stay (from 30 to 65 percent). Moreover, there was no relationship between those physicians who prescribed the longest period of bed rest and those who prescribed the longest total hospital stay. Finally, an assessment of the frequency of complications in these physicians' MI patients failed to document significant case mix differences to account for these varying practices.

Phineas and Lovell (137) reviewed the decreasing LOS for MI patients at the Royal Melbourne Hospital in the early 1960's. Without adjusting for severity of illness, they reported no difference in 3-month mortality among groups of patients with differing lengths of stay.

Figure A-1.—Regional Trends in Length of Stay for Myocardial Infarction



SOURCE: *Vital and Health Statistics*, series 13, Nos. 2, 10, 14, 17, 19, 23, 26, 31, 41, 46, 55, 60, 64 (Washington, D.C.: National Center for Health Statistics, 1967-82).

Rose (147) reviewed the evidence available in 1972, before any of the RCTs were published, and concluded that a week of bed rest was all that was necessary and that hospital discharge could take place a "few days" after being allowed to walk on the eighth day. Figure A-1 shows that U.S. physicians were a good deal more conservative in their practice; the average LOS in the United States for MI patients was 15.6 days in 1972. Wenger and colleagues (177) surveyed U.S. general practitioners, internists, and cardiologists in 1970 and reported on the treatment pattern that "most physicians" reported. Their treatment of choice was strict bed rest for 3 days, a total intensive care unit stay of 4.5 days, up in a chair by the eighth day, walking by the 12th day and discharged by the 21st day. This report is a good deal more conservative than the actual practice reflected in figure A-1. However, no distribution of physicians is given, so one does not know how much variation in physician reports there was.

The lack of solid data establishing the optimal LOS for MI patients prompted some researchers to analyze clinical data, either retrospectively or prospectively, in an attempt to identify characteristics of a low-risk group that might be able to be discharged earlier than the practice of the time. Wilson and Pantridge (182) evaluated 466 MI patients in Belfast that had survived 3 days hospitalization and found that of those without certain risk factors (shock, serious arrhythmias, and high enzyme levels), lack of persistent ST segment displacement on the electrocardiogram was a good predictor of the absence of late occurring serious ventricular arrhythmias. They suggested that patients in this category, which constituted 26 percent of the total population of MI patients, might be candidates for discharge after only 48 hours, since only a single patient

in that subgroup experienced a late ventricular arrhythmia.

The most often studied criteria for early discharge are those developed by McNeer and his colleagues (117) at Duke University. They first observed in 1975 in an analysis of 522 consecutive patients with documented MIs that patients who had suffered a serious complication *after* the first 4 hospital days also had one *during* the first 4 days. The complications identified as serious were: death, ventricular fibrillation or tachycardia second or third degree AV block, pulmonary edema, cardiogenic shock, persistent sinus tachycardia or hypotension, atrial flutter or fibrillation, and extension of infarct. They also found that of the patients without complications in the first 4 days, there was no inhospital mortality during an average LOS of 17 days and a 6-month mortality of 8 percent. This compared with an inhospital mortality of 14 percent in the complication group and a cumulative 6-month mortality of 19 percent. In their original series, patients with uncomplicated MIs made up 51 percent of the total MI population.

The Duke criteria have been replicated in three retrospective studies. Worth and colleagues (183) studied 455 definite MIs in four Honolulu community hospitals. They found four patients in whom serious complications first presented themselves between the sixth and eighth days. Three of these patients died in the hospital. All of these patients had been admitted for recurrent MIs. Of the 182 first MIs without complications through day 4, there were no inhospital deaths. Actual LOS at the four hospitals varied in this study from 12 to 18 days for patients with uncomplicated MIs and from 15 to 20 days for those with complicated MIs. Patients with uncomplicated MIs comprised 51 percent of the total population of patients with MIs.

The same criteria were used to evaluate a patient population in a community hospital in North Carolina. Severance and colleagues (158) reported that 81 of 400 MI patients (20 percent) had no serious complications during the first 4 days of hospitalization. Only one of these patients later had a serious complication: this patient survived an extension of his infarct. LOS was 17 days in the uncomplicated group and 18 days for the remainder. There were no deaths in this group during a 1-month followup period. Finally, Skoulas and colleagues (165), applied the McNeer criteria retrospectively to 210 consecutive MI patients in a Kaiser hospital in northern California. Like McNeer, they found that all patients with serious complications had had one during the first 4 days of hospitalization. There were no hospital deaths among the group without complications during the first 4 days and only one death during a 6-month followup period. The 87 patients with uncomplicated MIs comprised 41 percent of the total. They spent the least amount of time in the hospital of any group studied thus far, an average of 7.9 days. The complicated patients stayed only an average of 11.2 days. These patients were treated during 1978-79, and figure A-1 shows that these LOS figures were consistent with the West's average LOS for MI patients of 9.7 days in 1979. Other studies (111,163) have attempted to find criteria for early discharge (or safe transfer from intensive care) but none of them has been as successful as the Duke criteria. Other investigators (124,140) have analyzed patients admitted with suspected MIs retrospectively in order to determine criteria for safe early discharge or transfer from intensive care, but these studies have not been replicated, nor their criteria applied prospectively.

One cannot conclude from these studies that patients without complications in the first 4 hospital days following a MI can safely be discharged after that time. None of these studies was a prospective trial of early discharge. In three of the four, LOS for the uncomplicated MIs was 2 weeks or longer, and no attempt had been made actively to discharge these patients earlier than their physicians thought appropriate. It is thus not at all clear that earlier ambulation in preparation for earlier discharge would not have proved disadvantageous. The fact that all of these studies used almost identical criteria and found similar results lends added weight to the potential reliability and validity of these criteria as predictors of good prognosis and, therefore, of candidates for early discharge. Better data are needed, however, in order to establish this proposition conclusively.

Eight studies (2,22,26,27,35,53,173,174) report the results of early ambulation and discharge programs without providing control data. Table A-1 summarizes the most important findings from these studies. It is very difficult to draw any definitive conclusions from these studies. First of all, none of them was performed in the United States. Second, the study populations varied considerably. Four included only men, and two excluded the elderly. Third, the protocols used by the individual studies also varied. Even if attention is confined to those studies published in the 1970's, the period of bed rest varied from 1 to 3 days. Ambulation began on day 4 to 6, and discharge was planned from day 7 to 14. Fourth, results are reported inconsistently.

Some studies report results for high- and low-risk groups, usually those patients able to be discharged according to the protocol are separated from those with complications to whom the protocol for early discharge could not be applied. Other studies report results for the entire MI patient population. Some

Table A-1.—Uncontrolled Studies of Early Mobilization and Discharge for MI Patients

Study[a]	Patient population	Deaths before ambulation (%)	Protocol	Results (% of those ambulated)				
				Inhospital		Followup (cumulative)		
				Death (%)	Nonfatal reinfarct (%)	Time	Death (%)	Nonfatal reinfarct (%)
1. Brummer, 1956	332 consecutive MIs	22	Bed rest 2 wks. (mean = 16 days) Walk day 15 Discharge day 21 (mean = 23 days)	1	0.4	6 mo.	8	9
2. Brummer, 1966	775 consecutive MIs	28	Bed rest 10 days (mean) Discharge day 19 (mean)	2	1	1 mo.	3	4
3. Adgey, 1969	102 consecutive MIs, under 70, survived admission, discharged ≤ 18 days	—	Discharged 18 days	—	—	2 wks.	0	0
4. Royston, 1972	200 consecutive males with MI	11	Bed rest 3 days (60% ≤5 days) Walk day 4 Discharge 11-14 days (97% < 14 days)	4	?	6 mo.	11	?
5. Tucker, 1973	342 consecutive males with MI	15[b]	Bed rest 24 hr. Walk day 6 Discharge 7-10 days (mean = 8 days)	?	2	6 wks.	22	4
6. Chaturvedi, 1974	275 consecutive MIs surviving 6 days in hospital	—	Low risk (68% discharged by day 7)	?	?	3 mo.	0	?
						1 yr.	2	?
			High risk (64% discharged by day 11)	?	?	3 mo.	6	?
						1 yr.	11	?
7. Gelson, 1976	405 consecutive males with MIs	15[c]	Bed rest 24 hr. Walk day 6	?	?	6 wks.[d]	7	3
			Discharge 7-8 days (76% by day 8)			6 wks.[e]	8	16
8. Thornley, 1977	142 consecutive males under 65 with MIs	11	Bed rest 2 days (mean = 5 days) Walk day 4 Discharge 10-14 days (mean = 15 days)	2	?	24 wks.	6	10

[a] See Reference list for complete citations of studies in table.
[b] No data on deaths before ambulation. Figure given is all inhospital deaths. Followup data are given as percent of total patient population.
[c] No data on deaths before ambulation. Figure given is all inhospital deaths.
[d] Of 271 patients discharged by day 8.
[e] Of 87 patients discharged after day 8.

report results in terms of only those patients that could be ambulated, others for the entire group. All of these difficulties combined make it extremely difficult to interpret the data provided by these studies. For example, all of the studies that report death during inpatient ambulation found that some patients died during this process (1 to 4 percent). Would these patients have benefited from a longer period of bed rest? Or would more have died as the result of complications of bed rest? Without control groups carefully selected so as to be comparable to the study groups, these questions cannot be answered. Similar questions can be raised concerning the data reported on outpatient mortality, which ranged from a low of zero in 2 weeks, to a high of 7 percent in 6 weeks. Why did the 7 percent result occur in studies which employed only 24 hours of bed rest when another study with 10 days bed rest found a 1 percent outpatient mortality? Again, answers to these questions cannot be derived in the absence of control populations.

Five studies (21, 66, 69, 102, 116) reported the results of early discharge programs for MI patients and also reported results for control groups that were selected in a nonrandom manner. In the first such trial, Groden (66) reported on 105 men with MIs. Patients were allocated to the early or late discharge groups based on which of two consultants cared for them. Early discharge consisted of bed rest for 2 weeks, mobilization on day 15 and discharge on day 22. Late discharge consisted of bed rest for 3 weeks, mobilization on day 25, and discharge on day 36. Survival data were presented only for the period of hospitalization: 18 percent of the early discharge and 22 percent of the late discharge group died prior to discharge. Clearly,

these two groups may have differed in more ways than the treatment they received. Selection of a control group from among patients of a different physician than that of the study group is a less than adequate research design. Moreover, the lack of provision of outpatient mortality data is another serious defect.

Harpur and colleagues (69) reported a study in which patients admitted to one hospital were mobilized after 7 days of bed rest and discharged on day 15 and those admitted to a second hospital were mobilized after 3 weeks of bed rest and discharged on day 28. The two hospitals received admissions on alternate weekdays and weekends. In addition, the hospitals changed roles in the study at 4-month intervals for a 2-year period in order to ensure that both early and late discharge programs were carried out by both institutions. This study found that mortality at 8 months was 5 percent in the early group and 8 percent in the late group. Nonfatal complications were equally distributed. The two groups returned to work at about the same rates (about 75 percent of those eligible), but the early group returned about 2 weeks sooner than the late group.

Boyle and colleagues (21) reported on a group of MI patients who were discharged from their hospital within 10 days. They compared the experience of this group with that of patients remaining in the hospital more than 10 days. They found that after adjusting for severity of illness, there was no difference in 3-month mortality for the intermediate severity group for those patients discharged in less than 10 days compared with those who stayed longer than 10 days. They also found nonsignificant differences for the other severity groups and concluded that their program was not harmful.

Some of the problems associated with choosing a control group in a nonrandom fashion have already been discussed. In the Boyle study a new problem arises. The data presented are quite consistent with the hypothesis that early discharge is harmful. The argument is as follows: One would expect that of a total population of MI patients those who would be discharged early would be the ones with less severe infarcts. Thus, one would expect those discharged early to experience a lower mortality rate than those more complicated patients who were unable to be discharged early. Therefore, a study that found, as this one did, that its early discharge patients had the same mortality experience as their late discharges might be appropriately subject to the criticism that its early discharge program had in fact been harmful to those low-risk patients who participated in it. The only way to produce data not subject to arguments of this kind is by the employment of a random allocation strategy of study and control subjects.

Lamers and associates (102) described a unique study in 1973. They tested the difference between mobilizing MI patients on the 10th hospital day as opposed to the 20th day, while holding total LOS constant at 30 days. This study is included in the present discussion, because its report does not explicitly state that subjects were randomly assigned. They may have been, but no statement to this effect appears in the published report. In this study, patients were evaluated for possible inclusion on the ninth hospital day. This procedure eliminated 119 of the 555 patients admitted with definite MIs who had died before day 9. An additional 148 were eliminated because of the presence of complications, and 86 were eliminated because of "statistical problems." Thus, 202 patients (36 percent) were assigned to early and late mobilization groups.

The study protocol involved a graded schedule of mobilization beginning with dangling the legs over the side of the bed, then sitting in a chair, and finally beginning to walk only 4 days into the schedule. Thus, the early group was not actually ambulatory until day 14, the late group not until day 24. Compared with the programs summarized in table A-1, this is a very conservative early mobilization scheme. There were no inhospital deaths in either study or control groups. At an average followup period of 18 months, the early mobilization group had experienced a mortality rate of 17 percent, while 15 percent of the late group had died. The question that this study tried to test is an interesting one: What is the effect of early ambulation on MI patients, independent of total hospital LOS?

Unfortunately, several study design and reporting flaws make the results of the Lamers study difficult to interpret, even leaving aside the question of whether or not the subjects were randomly assigned. First, the study did not really test "early" ambulation as that term has now come to be understood. The previously described programs aimed for ambulation between days 4 to 6. Thus, the study was testing treatments that, even in the late 1960's, were not considered by most U.S. physicians to be innovative. Second, the study failed to report its followup data in adequate form. Life table analysis must be used if participants have not all been followed for the same time period. The reported data do not allow one to determine how many person-years of risk were contributed by each group. Thus, the outcome data are somewhat difficult to interpret.

Finally in this group of nonrandomly controlled studies, McNeer and his colleagues (116) have described an early discharge program in which they applied their own criteria in a prospective fashion. Of 158 consecutive MI patients, 67 had none of the complications previously described by the fifth hospital day. All 67 of these patients (42 percent of all patients

with MIs) were candidates for early discharge according to the criteria, but only 33 were actually discharged at 1 week. In 33 of the remaining 34 cases, the reason for the lack of an early discharge was either a home too distant for the followup nurse visits planned by the study or a home environment not conducive to MI convalescence. The patients who were discharged at 1 week were visited by a specially trained nurse practitioner equipped with a transmitter for cardiac rhythm monitoring every other day for the first week and every third day for the second week following discharge. The study reported no deaths in either subgroup of the 67 patients with uncomplicated MIs either at 3 weeks or at 6 months of followup. There were five nonfatal complications at 6 months in the early group and nine in the late group.

This study and its accompanying editorial generated some lively correspondence. The editorial commenting on the study (149) concluded that: "It is now clear that certain patients designated as 'low risk' can be discharged from the hospital at the end of one week." The correspondence that followed (125) both criticized and praised the study's design and findings. There is first of all the issue of the selection of the control group. Once again the lack of a random assignment subjects the study to several lines of criticism, each concluding that the study and control groups might have differed in ways other than their LOS. Even though the groups appeared well-matched when the usual demographic and clinical variables were assessed, the late subgroup did experience more late nonfatal complications (26 v. 15 percent). This could indicate that they were somewhat sicker than the early group at the outset, or that their less optimal home environment predisposed them to poorer outcomes, or that their longer hospital stay somehow made their outcome somewhat worse. There is no way to answer these questions in the absence of a random assignment design.

A much more serious question is raised by the small sample sizes in the early and late groups in this study. In part, the question is what can be concluded by a demonstration of no statistical difference between two experimental groups or, more precisely, by a failure to reject the null hypothesis. In this study, for example, if it is assumed that the zero percent mortality rate at 6 months in the late discharge group is correct, then if the true 6-month mortality rate in the early group is in fact 5 percent, this study stood a 63-percent chance of being unable to recognize it at the 5-percent significance level. The "power" of the study in this circumstance was, therefore, only 0.37.* If the true mortality rates were 1 percent in the late group and 6 percent in the early group, the power of this study to reject the null hypothesis of no difference between the two rates would be only 0.30. The small sample size also prevented the difference in the rate of nonfatal complications noted above from attaining statistical significance at the 5-percent level. The problem of small sample sizes will be discussed later in greater depth.

These technical issues aside, the fact remains that neither the early nor the late subgroup experienced any deaths at 6 months of followup. This fact certainly means that McNeer and his colleagues have been successful in identifying a low-risk subgroup of patients who came for care to the Duke Coronary Care Unit. It is also true that this study produced the lowest mortality rate reported for any study reviewed in this analysis. No other group has reported a 6-month mortality rate for its low-risk MI patients that is this low. This observation raises the question of whether the Duke population of MI patients is somehow different from others or whether their combination of treatments is somehow peculiarly successful. Unfortunately, these questions cannot be resolved until further prospective studies, hopefully RCTs, have been done.

The last group of studies to be reviewed includes 5 RCTs that have evaluated early discharge for MI patients. Their results are summarized in table A-2. The first (86) and most well known, was conducted in Boston and randomly assigned patients with uncomplicated MIs to 2- and 3-week LOS subgroups. The early discharge group was ambulated on day 12, the late group on day 17. It is noteworthy that only 17 percent of MI patients who survived to the day of assessment for participation in the study (day 5) were selected for randomization. Of those survivors rejected, 90 percent were excluded due to complications of infarction, due to a MI within 6 months of the current admission, or due to other medical illnesses. The early and late groups were fairly well matched, except that significantly more of the late patients had experienced previous angina than the early patients (23 v. 7 percent) ($p < 0.05$, chi square). Postrandomization complications prolonged the hospital courses of seven early patients and three late patients, but actual LOS figures were not reported for the two groups. There were no inpatient deaths. None of the outcome differences listed in table A-2 was statistically significant at the 5-percent level using the chi-square test.

The second reported trial (118) was carried out in Scotland and excluded patients over 70 years of age and patients with complications as assessed on the seventh hospital day. A far greater proportion of patients was included in this study (69 percent) than in the previous one (17 percent). This study also differed

*All power calculations performed here are done using the binomial approximation for proportions, a method which overestimates power slightly for very small proportions.

Table A-2.—Randomized Clinical Trials of Early Discharge for MI Patients

Study[a]	Exclusions	Percent survivors included	Protocols and (sample sizes) Early (E)	Late (L)	Followup period	Outcomes (%) Mortality E	L	Nonfatal complications E	L	Return to work E	L
1. Hutter, 1977	Complicated MIs assessed on day 5	17	Walk day 12 Discharge day 14 (69)	Walk day 17 Discharge day 21 (69)	6 mo.	4	7	26	22	58	49
2. Glasgow, 1973	Over 70 years complicated MIs assessed on day 7	69	Walk day 7 Discharge day 21 (269)	Walk day 21 Discharge day 28 (269)	12 mo.	11	15	10	10	—	—
3. Hayes, 1974	Complicated MIs assessed on day 3	71	Walk day 4 Discharge day 9 (107)	Walk day 9 Discharge day 16 (82)	6 wks.	7	7	6	5	2	2
4. Bloch, 1974	Over 70 years complicated MIs assessed on day 3	80	Walk day 8 Discharge day 21 LOS = 21 days (77)	Walk day 22 Discharge day 28 LOS = 33 days (77)	11 mo.	6	10	12	14	—	—
5. Ahlmark, 1979	Over 70 years complicated MIs assessed on day 4	75	Walk day 6 Discharge day 8 (128)	Walk day 12 Discharge day 15 (124)	3 mo.[b]	5	2	9	12	35	23

[a]See Reference list for complete citations of studies in table.
[b]All outcome percentages for this study are based on the number of patients in each group who did not suffer inpatient complications that prevented them from being ambulated or discharged on the target dates (early group = 106 patients; late group = 100 patients) (see text).

from the previous one in that those patients randomly assigned to the early discharge group were permitted to walk at an earlier time (day 7 v. day 12) but were kept in the hospital for a longer total time (21 v. 14 days). Those allocated to the late group were both ambulated later and discharged later than their counterparts in the previous study. Once again the two groups were well matched with one slight exception—the early group contained slightly more males than the late group (83 v. 74 percent; chi square, $p < 0.05$). As with the previous study, however, actual LOS figures were not reported. Unlike the previous study, however, this study showed significant inhospital mortality, 5 percent in the early group and 4 percent in the late group. The remainder of the cumulative 1-year mortality displayed in table A-2 occurred after discharge. None of the differences in outcomes was statistically significant at the 5-percent level.

Hayes and colleagues (71) reported a study from England that assessed MI patients on the third hospital day and excluded 29 percent of those patients surviving to be assessed as too ill to participate in the trial. In this study, patients were randomly allocated to early and late mobilization groups but were then sent to different hospital wards, based on a monthly schedule. Each hospital ward alternated on a monthly basis treating first early, then late patients, or vice versa. This design feature was employed in order to avoid having both early and late discharge patients on the same ward at the same time, apparently in order to avoid nursing confusion. Unfortunately, this scheme broke down during the trial when some wards became too full to accept patients. As a result, some patients were unable to go to the ward to which they had been randomly assigned. More unfortunately, when this occurred, the patients were sent to the ward with the most empty beds. Since this ward was most often a ward practicing early discharge, more patients were allocated to the early discharge group than to the late discharge group (107 v. 82). It is thus clear that the random assignment procedure in this study was seriously flawed. The early mobilization and discharge group in this study was treated with the earliest ambulation of any study in this group (day 4). The late group was treated with the earliest ambulation of any late study group (day 9). The two groups were fairly well matched on the following variables: age, sex, duration of pain, average blood pressure, site of infarct, and average enzyme levels. The 6-week mortality rate given in table A-2 conceals the fact that in this study all of the mortality in the late group occurred during the hospital stay, while four of the seven deaths in the early group occurred prior to discharge. There were no statistically significant differences between the two groups in the overall outcome measures.

The fourth RCT was performed in Switzerland and reported by Bloch and colleagues (17). Patients with documented MIs were assessed on the third hospital day and those with uncomplicated MIs who were also under age 70 were randomly assigned to early and late mobilization groups. Only 20 percent were excluded, the lowest figure of all the RCTs. The protocols for treatment in this study were very comparable to those used in the Glasgow study, with ambulation on day 8 and 22 for the early and late groups respectively and discharge on day 21 and 28. The actual lengths of stay for the early and late groups were 21 and 33 days. Again the groups were well matched. There was significant inpatient mortality in this study, with the rates being 5 percent and 6 percent in the early and late groups respectively. As with prior studies, no statistically significant differences were observed in the outcome variables listed in table A-2.

The fifth RCT is from Sweden, reported by Ahlmark and colleagues (5). They assessed patients on the fourth hospital day and excluded those over age 70 and those with complicated MIs. They randomly assigned 75 percent of the patients surviving to day 4 to early and late discharge groups. The essentials of the protocol are given in table A-2 and are similar to those of Hayes. Unfortunately, the researchers in this study compromised the random assignment process in a serious way. They excluded from the study all patients who had complications during hospitalization that precluded the possibility of discharging them at the appropriate time, day 8 or day 15. It is reported that 24 patients in the late group and 21 patients in the early group were so excluded. Even worse, followup data are not provided for these patients, making it impossible for the reader to add them back into the analysis retrospectively. The number of inhospital deaths is not reported for this group of excluded patients, although it is reported that one patient in the early group died in bed 4 days after admission while undergoing the early phases of the early mobilization protocol. This is the reason that the outcome measures reported for this study in table A-2 are given in terms of percent of patients discharged, instead of patients randomly allocated as is the case with the other studies. As with the other studies, no significant differences in the outcome measures were noted.

Before analyzing these studies further, a word is necessary about two other RCTs that compared home and hospital care for a selected sample of MIs initially determined to be uncomplicated (82,113). These studies have not been included in the present analysis

for two reasons. First, they are not directly concerned with the issue of hospital LOS and health outcome. The question of the most appropriate institutional setting in which to care for particular kinds of patients is a question separate from the one presently under consideration. Secondly, both these studies were done in England. The present social and medical-legal pressures that exist in the United States make it practically impossible either to perform such a study in this country or to care for any significant number of MI patients at home. Thus, this medical option is not viable in the United States at the present time.

From both clinical and health policy perspectives, the most important question that these RCTs can answer is: Does early discharge carry with it a negative health impact? A well-designed and well-executed RCT should be able, within certain limits, to provide the answer to this question. It is evident from the previous description that the third and fifth studies in table A-2 had their random assignment procedures sufficiently compromised that they cannot be considered true RCTs. They will be discussed later. The remaining three studies all showed similar results. The early discharge group fared slightly better than the late group in each of the three studies with respect to mortality (17, 86, 118) The studies also have in common the fact that none of the differences was statistically significant. There are also important differences among the studies. Two of the three excluded elderly patients, and one included only 17 percent of those patients who were evaluated for possible participation. Each of the studies assessed their patients for possible inclusion on different days, and each used somewhat different exclusion criteria and mobilization protocols.

What, then, can be concluded regarding the health impact of early mobilization and discharge for MI patients? After sifting through all of the studies that have been reported and narrowing the field down to these remaining three RCTs, the most rigorously designed and executed of all the studies that have been done, what conclusions can be drawn? The most important conclusion that can safely be drawn is that early mobilization and discharge, when applied under the terms stated in these studies, clearly does not pose a major health hazard to patients with uncomplicated MIs, as defined in these studies.

The issue of statistical power must still be discussed. Even the study with the smallest sample size (i.e., the first study in table A-2 with samples of 69 in each group) has an excellent chance of finding a large difference in mortality between the early and late groups. For example, assuming the control or late group had the same mortality as it actually had (7 percent), if the study or early group had a true mortality rate of 30 percent, the study design actually employed would have had only a 3-percent chance of making a Type II error at the 5-percent level of significance and falsely accepting the null hypothesis of no difference. The power of the design under these circumstances would be 0.97. However, the smaller the difference that is of interest, the greater the chance of making a Type II error. For example, most physicians would certainly agree that if the true mortality rate for uncomplicated MI patients discharged early were 10 percentage points greater than for those discharged late, none should be discharged early. This same study, again assuming a 7-percent mortality rate for the late group, has a power of only 0.57 if the true mortality rate of the early group is 17 percent. Thus, one could expect to make a Type II error barely less than half the time in trying to observe a difference in mortality rates of this magnitude with this study design.

Table A-3 displays some similar power calculations for the three true RCTs just identified. It is clear that while all of these study designs are sufficiently robust to detect large differences, none is especially powerful in trying to detect a difference of 5 percent. From a clinical perspective, it is certainly prudent to perform an experiment with small sample sizes first to rule out the possibility of a large negative effect before proceeding to a large trial to evaluate the possibility of much smaller negative or positive effects. However, such an initial experiment may not provide sufficient evidence by itself to justify adoption of the experimental treatment.

Another way to analyze the data is to take a closer look at the actual study results instead of hypothesizing about possible results. For example, one can construct 95-percent confidence intervals for the difference between the mortality rates in the early and late groups in each of these RCTs. In doing this, one finds that the data in the first study are compatible with differences ranging from 11 percent in favor of the early

Table A-3.—Power of Randomized Clinical Trials on Early Discharge for MI Patients

Study[a]	Statistical power under three alternative hypotheses		
	5%[b]	10%[c]	20%[d]
1. Hutter, 1977	0.26	0.57	0.94
2. Glasgow, 1973	0.45	0.90	0.9999
3. Bloch, 1974	0.25	0.53	0.94

[a]See Reference list for complete citations of studies in table.
[b]Chance of appropriately rejecting the null hypothesis of no difference at the 5-percent significance level if the true mortality rate for the early group was 5 percentage points greater than the actual rate experienced by the late group.
[c]Chance of appropriately rejecting the null hypothesis of no difference at the 5-percent significance level if the true mortality rate for the early group was 10 percentage points greater than the actual rate experienced by the late group.
[d]Chance of appropriately rejecting the null hypothesis of no difference at the 5-percent significance level if the true mortality rate for the early group was 20 percentage points greater than the actual rate experienced by the late group.

group to 5 percent in favor of the late group. The second study varies from 9 percent in favor of the early group to 2 percent in favor of the late group. And the third study varies from 13 percent in favor of the early group to 5 percent in favor of the late group.

The best data available to answer the question of the effect of early discharge on the health status of uncomplicated MI patients suggests that it is most unlikely that a large negative health impact will ensue. The data do not, however, exclude a small negative impact, on the order of a mortality rate at 6 months to 1 year that is 5 percentage points greater in the early than in the late group. Many clinicians would undoubtedly feel that this is a clinically significant risk. The data are also consistent with the possibility that early discharge is associated with a modest (about 10 percent) decrease in mortality. A much larger study than any of these RCTs would have to be designed in order to settle the issue definitively. One additional point should be raised in the context of the Medicare program. Since two of the three RCTs excluded patients over age 70, as did a number of the other less sophisticated studies, one must admit that there is a special dearth of data from which to draw any informed conclusions with respect to the elderly and early MI discharge.

Is there any additional information to be gleaned by adding the data from the studies previously reviewed? While accepting the fact that they are less rigorous than the RCTs, one can look generally at the results obtained by the five nonrandom controlled studies and the two flawed RCTs (5, 21, 66, 69, 71, 102, 116). Across all of these studies the mortality rate for the early group varied from 0 to 18 percent and that for the late group from 0 to 22 percent, with a followup period that varied from 0 to 18 months. In two of the studies (5,102), the early group experienced a greater mortality at followup than the late group; in three (21,71,116), the mortality rates were equal; and in the remaining two (66,69), the late group experienced a greater mortality rate than the early group. This group of studies appears to come down squarely on the middle of the fence between early and late discharge.

Going back to the studies summarized in table A-1 is equally fruitless. One can conclude that some of these research groups do indeed appear to have identified groups of MI patients at low-risk for early discharge, with mortality rates comparable to those seen in the RCTs. But these data shed no further light on the question of whether these low-risk patients would have done even better with longer periods of bed rest and hospitalization. In addition, as in the RCTs many of the earlier studies excluded the elderly, and some excluded females. These studies do not therefore help to bridge the information gap for these population subgroups.

One final comment is in order. It has been concluded from this review that the best data on early discharge for uncomplicated MI patients demonstrate that 3 weeks hospitalization is not a lot worse than 4 weeks (17,118). There is also some evidence, albeit somewhat less sturdy, that 2 weeks is not a lot worse than 3 weeks (86). Figure A-1 demonstrates, however, that the U.S. average LOS for all MI patients was down to 12.6 days in 1980 and down to a mere 9.6 days in the West. Unless there are massive problems with diagnosis coding, the LOS for patients with uncomplicated MIs must be even briefer. U.S. physicians may have adopted an early discharge policy for MI patients that is more aggressive than a conservative assessment of the available data would justify. Does this imply that medical practice today is reaching the opposite extreme to that of medical practice 40 years ago? Can a series of editorials be expected soon decrying the abuse of early ambulation and discharge for MI patients? Additional research is required before an optimal LOS for MI patients can be defined.

Appendix B.—Length of Stay and Outcome: Elective Surgery

> Of late I have allowed my patients to get up within twenty-four to forty-eight hours and to leave the hospital four to six days after their vaginal celiotomy. I could not fail to notice that these same patients did not present the picture of listlessness and muscular weakness which the same category of patients present after the performance of the same operations by the abdomen with the usual after-treatment (143).

Emil Ries began the movement toward encouraging postoperative patients to walk within the first day or two after surgery with this statement at a meeting of the American Medical Association in 1899. In the same paper, Ries also advocated early postoperative feeding as another means to speed recovery. Although he practiced these principles throughout his career and although this paper received a positive reception, Ries did not influence the majority of surgical practice, which continued to employ long periods of bed rest following surgery.

Although a few other voices were heard in support of "early rising" and even outpatient surgery (131), it was not until the 1930's that this practice was revived in the United States. Leithauser (106) summarized the experience of others, largely European and Russian, and tallied 15,000 reported cases of early postoperative ambulation with only four "fatal emboli." He showed that the well-documented postoperative decrease in vital capacity (36) improved with early ambulation. He also presented a personal series of 900 patients whom he treated with an average of 1.3 days of bed rest and 4.0 days of total hospital stay. He claimed that his patients did not show a greater than usual incidence of wound dehiscence or infection.

In the 1940's and 1950's, a series of nonrandom controlled studies appeared (18,19,28,59,136,141). Each of them compared a group of surgical patients who had been encouraged to ambulate within a few days of surgery to those who had remained in bed for longer periods of time, frequently over a week. One study (28) analyzed patients from two hospitals, one of which practiced early ambulation. Only patients who had undergone abdominal surgery were examined. The patients at the hospital practicing early ambulation experienced a rate of wound disruption of 0.05 percent while the patients treated more traditionally had a rate of 1.05 percent. The other studies were similar in their use of control populations other than those created by a random allocation procedure. All of the studies found fewer complications in the group that ambulated early, including no increase in recurrences after herniorrhaphy (19).

Even though these studies are subject to the usual criticisms of studies employing nonrandom controls, they were apparently very influential. By the 1950's, early ambulation for surgical patients was a well-established principle of surgical management. Editorial writers (8,23,103) routinely warned of the dangers of too much bed rest. As they often took pains to point out, however, early ambulation did not mean early discharge (23,103).

As in the myocardial infarction (MI) literature, since the 1950's three different kinds of studies have been performed. The first group comprises data analyses, studies which have examined length of stay (LOS) differences and tried to explain them. Second, there have been a large volume of uncontrolled trials of early ambulation and discharge. And third, a number of randomized clinical trials (RCTs) have been carried out. Each of these groups of studies are reviewed and summarized, with particular attention to RCTs.

Early in the 1960's, many researchers in Great Britain noted that LOS there was much higher than in the United States and Western Europe. Stallworthy (168) criticized a lack of efficiency in British hospitals and called for a decrease in LOS generally accompanied by experiments to document the increased efficiency which he believed would be obtained in the presence of shorter lengths of stay. He wrote (168):

> Any major reform is certain to challenge many traditional concepts and may arouse powerful opposition. Experiments with pilot schemes can be valuable; for once facts prove a contention it is difficult for opposition to survive.

Jones (92) commented that although British LOS had fallen during the 1950's, there was room for further declines. Heasman (75) noted regional differences in British LOS for tonsillectomy (2 to 6 days) and herniorrhaphy (8 to 12 days). She also saw a need for better data on the relationship between LOS and outcome (75):

> Statistically controlled studies are needed to show objectively the effect of different lengths of stay in hospital for uncomplicated cases.

Analyses of LOS differences for surgical patients continued into the 1970's (64,65,119,157,178). These studies continued to show large differences in LOS among surgeons and hospitals, but none of them were successful in building models to explain these variations. One study (119) assessed LOS by surgeon for uncomplicated cholecystectomy at the Marshfield Clinic. This study found that postoperative stay varied among the five surgeons from 5.3 days to 7.3 days.

No statistically significant differences were found among surgeons with respect to patient outcome or quality of care, although the surgeon with the longest LOS did have the best outcomes. Seventy-six percent of his patients were asymptomatic and had returned to normal activities, compared with 61 percent of the patients of the surgeon with the shortest postoperative LOS.

The most common type of study in this literature is the uncontrolled trial of early ambulation or early discharge. This usually takes the form of a single surgeon reporting his or his hospital's experience with a particular scheme of this kind of postoperative management. No less than 14 such studies have appeared since Farquharson (49) reported the first large series of herniorrhaphies performed on outpatients in 1955.

Before reviewing the data from these analyses, a discussion of the limits of the present analysis with respect to surgery is appropriate. The logical extension of early discharge for surgical patients is outpatient surgery. This subject will be included in the analysis to follow. However, there are many ways in which to do outpatient surgery. The model that will be discussed here is one in which the only difference in the treatment received by inpatients and outpatients is that the outpatients are discharged without spending a night in the hospital and receive much of their postoperative care on an ambulatory basis after discharge. The same staff and facilities that provide surgical services to inpatients also provide the same services to outpatients. Excluded from this analysis are studies in which both type and place of treatment were varied—e.g., the RCT comparing inpatient surgery and outpatient injection therapy for varicose veins (34,12).

This analysis will also not evaluate the establishment of a separate facility designed solely for the provision of outpatient surgery (e.g., a surgicenter). Just as it was beyond the scope of the present analysis to consider the appropriate place of service for provision of services to MI patients (home v. hospital), so is it beyond its scope to consider the most appropriate place of service for surgical patients. This is a very complex question, involving questions of physician training, ancillary staffing levels, nature of anesthesia used, equipment availability, resuscitation capabilities, and other factors. This discussion will be limited to an evaluation of data pertinent to the question of how a hospital can best provide surgical services to its patients.

Equally beyond the scope of the present discussion is the question of the appropriateness of the surgery itself. This is not an analysis of the necessity of surgery as opposed to other treatment modalities. The rest of this chapter will try to answer the following question: Once the decision that a patient should undergo surgery in a hospital has been made, what is the relationship between LOS and health outcome?

Farquharson (49) recalled Ries' work and described a series of 485 patients on whom he had performed inguinal herniorrhaphies under local anesthesia and then discharged. His description of this outpatient surgical procedure merits repeating:

> As a rule the patient is little disturbed by the operation. He climbs down from the operating table, walks out of the theatre, dresses in his lounge suit, and then walks out to the ambulance in which he is taken home. Our aim is to get him back to his own bed while the local anaesthetic is still effective.

The usual procedure for inguinal herniorrhaphy patients at that time was 5 to 6 days bed rest (down from 21 days in the early 1940's) and 10 days hospitalization. Farquharson selected patients with home environments conducive to home convalescence, though he gave no objective criteria. He noted a decreasing complication rate, 10 readmissions to the hospital for complications in the first 285 patients, but only 1 in the last 200 patients. He reported "some" recurrences without giving actual numbers but asserted that his experience was "at least as good" as that claimed by supporters of more traditional approaches. He also wrote that patients were satisfied with the novel procedure and that one of the most important benefits of the outpatient strategy, aside from monetary saving, was the dramatic reduction in waiting time that was possible. At that time, considerable waiting lists had built up in Great Britain for elective surgical procedures. Sicker patients were admitted to scarce hospital beds ahead of candidates for elective surgery, who were called in for their procedures when inpatient beds and operating time were available. Eliminating the need for a 10-day hospitalization enabled surgeons to operate on more patients during a given time period.

This study establishes the parameters by which all succeeding work may be judged. First, one must recognize that the scope of this study is narrower than those cited previously. Previous studies discussed the early ambulation of essentially all surgical patients. This study and succeeding studies are concerned with even earlier ambulation and discharge of a selected subgroup of elective surgery patients, typically patients with inguinal hernias, varicose veins, and hemorrhoids. Second, outcome measures are difficult to define and measure. Mortality is vanishingly small (122) so other measures must be sought. For hernia patients, the one most clinically important is the recurrence rate. This too is often quite low (under 1 percent) (122). It may also be difficult to compare figures from one study to another. In order to evaluate these figures, it is necessary to know precisely how many patients were fol-

lowed over what time period; one must be able to determine the population at risk for recurrence. Postoperative complications are another possible outcome measure. To be useful, however, careful definitions must be constructed and followed. Patient satisfaction may also serve as an outcome measure, but again careful attention to reliable and valid measurement is important to the production of accurate data. Finally, one may wish to assess the monetary impact of early discharge programs. Since all of the studies that attempt to do this were performed outside the United States, these exercises will have only slight relevance to U.S. health policy. They will serve to illustrate, nevertheless, how difficult such analyses are to perform.

Farquharson (49) discussed all of these issues but provided data on only one, complications requiring admission to hospital. Without having a control group with which to make comparisons, these results cannot be judged good or bad. Stephens and Dudley (169) reported data similar to the previous study in 1961. Of 212 patients on their waiting list for hernia or varicose vein surgery, they selected 164 (77 percent) as candidates for outpatient surgery, excluding those over age 70, with complicating medical problems, or with homes too distant or incompatible with immediate postoperative convalescence. No specific criteria were given regarding the medical exclusions or what constituted an unsuitable home environment. These patients were operated under general anesthesia and discharged 5 to 8 hours later. The initial recovery from anesthesia took place in a general ward bed. These authors reported no serious postoperative complications, though there was a high incidence (27 percent) of nausea and vomiting until the premedication schedule was altered (171). Recurrence data were not reported. Patients reported a high degree of satisfaction (96 percent) with outpatient treatment. No financial data were presented, but a significant impact was claimed in reducing the size of the waiting list.

Williams (180) described similar results with a small series of patients and raised an additional issue. He noted that the general practitioners in Great Britain were being asked to increase their workload as a result of outpatient surgery programs. Dean and Wilkinson (41) confirmed Williams' opinion that most general practitioners were in favor of selective outpatient surgery despite the increased visits required by it, chiefly because of its salutary effect on elective surgery waiting lists. Ruckley reported two series of patients, primarily those with hernias and varicose veins, who had outpatient surgery (152,153). Both groups of patients experienced a complication rate of 10 percent in the postoperative period, and 6 percent of the second group of patients could not be discharged as planned because of complications. An additional 2 percent of patients had to be readmitted after discharge because of complications. There were no fatalities, and the complications were not considered serious enough to entertain thoughts of discontinuing the program. No data on patient selection were given except that no patients over 68 years were included. The remaining uncontrolled study (11) of outpatient surgery added no new data.

The principal conclusion that may be drawn from this body of data on ambulatory surgery is that these authors have succeeded in identifying a subgroup of patients with inguinal hernias and varicose veins in Great Britain who can have surgery performed on an ambulatory basis with a very low rate of serious complications and an even lower mortality rate. However, in the absence of adequate control data, one cannot be sure that this same subgroup would not have done even better as inpatients.

The group of studies on early discharge for elective surgery follows a pattern quite similar to the group just reviewed on ambulatory surgery. Aldridge (6) reported a 1-percent complication rate with a discharge program at 48 hours postoperatively for herniorrhaphy patients. No selection or recurrence data are given, and high rates of satisfaction among patients, staff, and general practitioners were claimed. Hockey (84) reported the results of a program in which a nurse provided home followup care in order to permit early discharge for patients undergoing herniorrhaphy, appendectomy, and other surgical procedures. The author estimated, using usual LOS figures for her patient population, that 4.7 days per patient were saved as a result of the program. No complications could be identified that could clearly be related to the early discharge program, though 6 of 126 patients were readmitted in the postoperative period for unrelated problems. Again, high rates of satisfaction were claimed. No selection criteria were given.

One of Ruckley's reports (153) on outpatient surgery also contained a series of patients discharged early. He estimated a saving of 3 to 6 days per patient depending on the procedure using the same method as Hockey. No other data were given. Doran (45) reported on 705 patients discharged within 48 hours following herniorrhaphy and varicose vein surgery. These patients represented 77 percent of all patients evaluated, again with no criteria for acceptance specified beforehand. Only 5.4 percent could not be sent home early because of immediate postoperative complications, and an additional 9 percent developed late postoperative complications at home. None of these proved serious but 0.9 percent did require readmission.

Doran also reported high rates of satisfaction among patients and referring general practitioners, in large part due to reductions in waiting time for surgery. Chant and colleagues (33) reported a similar study of 105 herniorrhaphy patients with a complication rate of 10 percent. Cannon and colleagues (30) reported an early discharge study in unselected hernia patients and found that they were able to plan early discharge for 54 of 104 patients (52 percent) but only able to discharge 24 within that targeted period.

Finally, two reports from the Shouldice Clinic in Toronto (87,55) suggest that attainable rates of mortality and recurrence for elective herniorrhaphy are small indeed. The first report from 1965 (87) documented an operative mortality rate of 0.05 percent among 30,946 patients between 1945 and 1960. This Clinic treats only hernia patients and reported a recurrence rate of 0.6 percent. Although precise data on years of followup were not provided, many patients were followed for 10 years or more. The Clinic uses local anesthesia on adults and ambulates patients on the day of surgery with discharge 72 hours following the operation. In the first series, a wound infection rate of 1.8 percent was reported, but no complication data except that for recurrence were reported in the second.

The most serious deficiency of this group of uncontrolled studies on outpatient surgery and early discharge is the lack of an appropriate comparison group. The implications of this failing have already been discussed. In addition, without a strictly defined set of selection criteria, it is difficult to know precisely to which kinds of patients the results might apply. Similarly, without preset, uniform criteria for what constitutes complications, it is difficult to compare one series to another or even patients within a single series if more than one physician determines the presence or absence of complications. In this group of studies, complication rates ranged from 0 to 10 percent. How much of this variation is attributable to differences in implicit criteria among individual physicians? Finally, none of these studies reported hernia recurrence rates precisely in terms of person-years of followup.

Before discussing the true RCTs, two studies should be mentioned. Palumbo and Sharpe (135) describe a trial of early ambulation of herniorrhaphy patients in which patients were ambulated at different times postoperatively: at 0 to 1 days, 3 to 10 days, or after 10 days postoperatively. The study found lower rates of complications and recurrences in the earliest ambulated group. Unfortunately, this study is described briefly as part of a larger review and its methods are so inadequately delineated that it is impossible to ascertain the kind of research design that was employed. The word "randomized" is used in the abbreviated description of the study, but the sample sizes are so unequal that one wonders if random allocation could really have been used. It is also not clear in which of the three groups (if any) the patients who were ambulated on the second postoperative day belong. Finally, no LOS figures are given to allow one to determine whether this was an early discharge program as well.

Similar problems are present in the study reported by Kornhall and Olsson (101). They apparently compared a series of 54 patients operated on for hernia repair as outpatients with a matched sample of 54 patients randomly selected from among those who received their repairs as inpatients, with a mean hospital stay of 3.4 days, though the report's description of the study design is vague. This is thus a comparison of outpatient surgery with early discharge following inpatient surgery, a most interesting research question. Unfortunately, the small sample size and the lack of a truly random allocation procedure makes the result of no difference in complications difficult to interpret.

The true RCTs are summarized in table B-1. Patients in the first study (121) cited in the table were randomly allocated from a list of patients awaiting hernia repair after their general practitioners approved. The study group was discharged after 1 night in the hospital while the long-stay group was kept 5 to 6 days. No actual LOS figures are given, but it is reported that 10 of the 11 short-stay patients with complications prior to discharge were kept past the first postoperative day. There were no statistically significant differences in complications or recurrence rate (assessed at 1 year in three-fourths of the patients in both groups). The recurrence rate was 3 percent in the early group, 6 percent in the late. The study did document a significantly increased use of general practitioners and nurses postoperatively by the short-stay group (2.4 visits per person v. 0.6).

The second study was performed in Cali, Colombia (48). Its criteria for inclusion were very strict, resulting in the elimination of 82 percent of patients before random assignment. It appears from the report that the study design was needlessly complex. It required that patients who passed the inclusion criteria be matched on a large set of clinical and sociodemographic variables. Then, one member of each pair created by the matching was randomly assigned either to outpatient surgery or to regular inpatient postoperative care. Some of the eligible population was excluded, because no matched pair could be found. This helped to reduce drastically the fraction of patients available for study and rendered the results questionable since so many patients were eliminated prior to random assignment. Two other deficiencies compound this problem. First, patients were eliminated from study if there was an

Table B-1.—Randomized Clinical Trials in Outpatient and Short-Stay Elective Surgery

Study[a]	Study populations	Percent included	Protocols and (sample sizes) Early (E)	Late (L)	Complications (%) E	L	
1. Morris, 1968	Men, 21-65; hernia; no chronic disease; good home; referral	?	Discharge 1 day post-op (92)	Discharge 5-6 days post-op (93)	27	19	
2. Echeverri, 1972	1-59; hernia; varicose veins; no chronic disease; good home; no post-op complications	18	Discharge 3-5 hrs. post-op (44)	Regular inpatient (44)	5	7	
3. Gerson, 1976	No chronic disease; good home; multiple diagnoses	?	Discharge early to home care (399)	Regular LOS (184)	—	—	
4. Russell, 1977	18-70; hernia; hemorrhoids; no chronic disease; good home	60	Discharge 5-6 hr. post-op (55)	Discharge 5-6 days post-op (56)	45	30	
5. Simpson, 1977	Cholecystectomy; vagotomy; no chronic disease; simple surgery; good home	59	Discharge by criteria (53) (LOS = 7.6 days)	Discharge planned at 10 days postop (47) (LOS = 9.7 days)	23	26	
6. Adler, 1978	18-64; hernia; varicose veins; no chronic disease; good home	?	Discharge 2 days postop (117)	Discharge 6-7 days postop (107)	13	5	
7. Ruckley, 1978	Over 60 only if fit; hernia; varicose veins; no chronic disease; good home	74	Discharge 4-6 hr. postop (117)	Discharge at 2 days postop from ward (121) from conv[b] (122)	home 38	ward 24	conv[b] 39

[a]See Reference list for complete citations of studies in table.
[b]Convalescent hospital

intraoperative or immediate postoperative complication or if a blood transfusion was used. Thus, it is not known how many patients for whom outpatient surgery might be planned would be unable to be discharged immediately because of complications. Second, 13 of the 44 pairs were eliminated after surgery due to errors in selection and matching. Thus, data on convalescence are given for only 31 pairs. Complication data, however, are given for all 44 pairs. It is not clear why the methodological problems were sufficient to eliminate 13 pairs from one analysis but not the other. These methodological problems and the setting of this study —i.e., a socioeconomic environment so different from that of the United States—make its results of slight relevance to the present analysis.

The study reported by Gerson and Berry (54) has few design flaws but several important analytical ones. In this study, patients were randomly assigned to a study group that was eligible to receive home care following discharge if the attending physician so desired and to a control group that was treated by the usual hospital postoperative stay. The authors included a large number of surgical and a few medical conditions. These illnesses were selected after a preliminary study indicated that the selected conditions presented the best opportunity for the substitution of home care for inpatient care. The study excluded patients with multiple diagnoses or complications that might extend LOS and patients whose homes did not meet certain safety standards. Eligible patients were randomly assigned to study and control groups at a 2:1 ratio. The researchers either selected candidates poorly or did not have the enthusiastic cooperation of attending physicians, because only 176 of 399 home care candidates were actually referred to the home care program and, presumably, discharged early. The remainder stayed in the hospital for a usual postoperative or convalescent course.

Unfortunately, no LOS data are provided for the study and control groups. Data are provided only for those study patients who were discharged to home care and for all the remaining patients (study patients remaining in the hospital without home care referral and control patients). One cannot determine whether the program succeeded in reducing overall LOS for all the study patients without LOS data on the entire study group. This study apparently squandered the ability of random allocation to create comparable patient groups by failing to analyze the correct data.

Instead, the authors proceeded to analyze five surgical conditions for which the subgroup of study patients had shorter LOS than the remaining combined group. They found no differences in rates of return to work among the study patients who received home care, the study patients who did not receive home care, and the control patients. They also found somewhat better functioning at home in the study patients receiving home care and concluded that this might be a beneficial result of the program. This is a mistaken conclusion. While one might conclude that another study should be done the candidates for which would be drawn from among only those subgroups who actually experienced shorter lengths of stay in conjunction with the home care program, the study provides no information to suggest that the home care program was successful. It did not demonstrate that the home care program reduced overall LOS for eligible patients. From an analytic point of view, it is fallacious only to eval-

uate those subgroups of an experimental population which seemed to have derived a benefit from the experiment without also considering those apparently suffering a negative result. The authors appear to have done just that. It may well be that home care after hospital discharge can produce benefits as a treatment in and of itself. This study fails, however, in the effort to document that it can substitute for inpatient convalescent care.

The study reported by Russell and colleagues (154) is a trial of outpatient surgery and usual inpatient care for nonelderly patients with hernias and hemorrhoids. The requirements for absence of chronic illness and for adequate home support for early discharge that are typically present in these studies were also present here. Nine patients were eliminated after random allocation because of medical problems identified by family physicians (2) or inadequate home environments determined at preoperative interviews (7). Thirteen additional patients were eliminated from study because their surgery did not take place for a variety of reasons. Therefore, only 60 percent of the originally screened group entered the trial. The authors comment that although strict definitions of complications were not employed, researchers were encouraged to report all complications, "however slight." Due to small sample sizes the large difference in complication rate noted in table B-1 is not statistically significant at the 5-percent level ($p > 0.1$). The authors comment that the high rate of complications in the short-stay patients was the result of a large difference in the hemorrhoid patients, an occurrence they attributed to the postoperative use of a particular kind of dilator. They reported anecdotally a decrease in complications after use of this dilator was discontinued following the conclusion of the study. The study did document a significant difference in the number of visits made by patients to their general practitioners or district nurses. The short-stay patients made an average of eight visits per person during an unspecified period of followup while the long-stay patients made an average of four.

The study reported by Simpson and colleagues (161) was a well-designed and well-executed RCT. They studied the difference between employing a rigid notion of when postoperative discharge could occur (10 days) and the use of criteria to determine fitness for discharge. Ten days was chosen as the fixed day of discharge because it represented the modal discharge day for uncomplicated patients receiving the two operations studied here: cholecystectomy and vagotomy. The criteria included items describing healthy wound appearance, adequate feeding, and freedom from complications. The study demonstrated a significant reduction in average LOS for the criteria-based discharge group (7.6 v. 9.7 days) and a complication rate that was no different. This study thus documented that flexible, clinically based criteria for discharge can result in shorter lengths of stay for cholecystectomy and vagotomy patients when compared to a plan of discharge fixed at the mode.

The sixth study summarized in table B-1 (3,4) is a trial of short-stay versus more traditional stay, a design similar to the first RCT. This study excluded the elderly and accepted referrals from general practitioners if they considered their patients with hernia or varicose veins to be medical and social candidates for early discharge. Patients were randomly assigned to discharge at either 2 days or 1 week postoperatively, although no actual LOS figures are given. The difference in complication rate, which is almost entirely due to a large difference in complication rate for the varicose vein patients (0 v. 13 percent), is of borderline statistical significance ($p < 0.1$). All of the complications were felt to be of minor clinical significance, none apparently requiring readmission to hospital. This study also is the only one that measured hernia recurrences in terms of person-years at risk. The rates were an identical 0.02 per person-year at risk for the study and control groups assessed with an average followup of 2.3 years per patient.

This study also measured cost more carefully than any other. Although its direct relevance to U.S. policy is slight since it was carried out in Great Britain, the general findings are instructive. The authors found a definite saving in hospital costs. These savings were all but offset, however, by an increased cost in the study group due to longer time off from work and to increased costs to patients and families in the short-stay group. The net social saving was, therefore, slight. The difference in time from surgery to return to work was accounted for entirely by the difference observed in the male patients: 34.5 days for the long-stay group v. 38.2 days for the short-stay group. This difference is not statistically significant but turned out to be economically significant in the savings calculations. Patient satisfaction showed no differences between the two groups, but the families of the short-stay patients were significantly less pleased about the policy of early discharge than the families of the long-stay patients were about their relatives' stays. General practitioners approved the policy, despite the increase in their workload. Finally, this study is notable for having published a report that deals solely with questions of methodology and the technical difficulty of performing such a study (4).

The last study in this group (151) is a unique trial of outpatient surgery and short-stay surgery for hernias and varicose veins. The study assessed three different modes of postoperative care: home, inpatient surgical ward, and convalescent hospital. Patients were

randomly assigned to immediate home discharge (4 to 6 hours postoperatively), to 48 hours in hospital, or to 48 hours in a convalescent hospital. Also unique in this study is the fact that neither surgeons nor anesthetists were aware of which patients were assigned to which groups. The study had the usual exclusion criteria, not specified in detail prior to the study.

Significant differences were observed in complication rates, with the hospital ward patients experiencing the lowest rate of complications. The difference among groups in total complication rate is significant at the 5-percent level (chi-square 7.2, d.f. 2). Over half of the complications were accounted for by delayed wound healing. Only three patients assigned to the convalescent hospital and two home care patients required postoperative hospital stays because of operative or anesthetic problems; all were discharged 1 day postoperatively. Only three patients required readmission to the hospital during the followup period; all three had been in the hospital group. As in the previous studies in this group, the complications were regarded by the authors as "medically trivial," and the large majority were managed on an outpatient basis. Patient satisfaction was high, though precise data were not reported. Again, patients receiving outpatient surgery required more attention from local physicians and nurses than did patients kept in hospital.

Four of these seven studies tested outpatient short-stay surgery in various combinations and in a methodologically sound manner (3,121,151,154). The results are remarkably consistent. In each case, a substantial number of patients were identified who could undergo outpatient or short-stay surgery for their hernias, varicose veins, or hemorrhoids without serious complications. In each case, the long-stay group had fewer complications, but these were judged minor in all studies. The results of these RCTs and the other studies reviewed here have undoubtedly played a major role in the dramatic fall in LOS that has occurred in the United States for hernia patients since 1968. Figure B-1 depicts the decrease by region. The U.S. average LOS has declined by 35 percent, that of the Northeast by 46 percent, the Northcentral by 28 percent, the South by 21 percent, and the West by 42 percent. These data suggest that an increasing number of U.S. surgeons, particularly in the West, are discharging more of their inguinal hernia patients at an earlier postoperative date. Many of these patients were probably discharged on the second postoperative day, the most common target for the early discharge programs reviewed here. Are they right? Should discharge on the second day after inguinal herniorraphy be the rule instead of the exception?

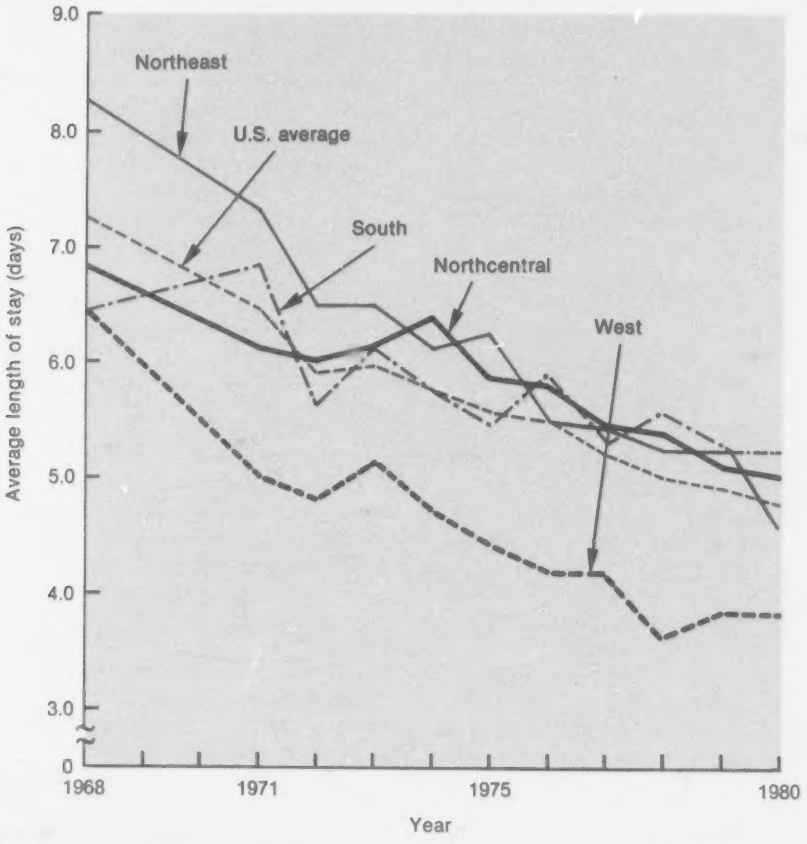
At this point, the issue of statistical power once again arises. Using herniorrhaphy as a model, it is clear that the two most important outcome measures from a clinical standpoint are operative mortality and recurrence rate. Operative mortality was reported in two studies reviewed here (87,135). It was the same in both of these large series: 0.05 percent. In order for a study to have even a 50-percent chance of distinguishing a doubling of this operative mortality, one would have to randomly assign over 16,000 patients each to study and control groups. It is thus highly unlikely that we will ever have comparative data on operative mortality from RCTs such as those reviewed here on which to base decisions concerning appropriate postoperative management. A sample size of 2,000 would be required before one would expect even one operative death. Clearly, studies with sample sizes of 90 to 120 cannot observe anything useful regarding surgical mortality in elective herniorrhaphy.

The story is similar, but not quite as hopeless, with respect to recurrence rate. Assuming a 2-percent recurrence rate (greater than that achieved by the Shouldice Clinic, but equal to that seen in one of the RCTs (54)) a study with a sample size of 120 would have only a 23-percent chance of rejecting the null hypothesis of no difference if the true recurrence rate in a study population of short-stay patients was 4 percent. If the sample sizes were increased to about 400 in each group, the power would increase to 0.5, or to 0.7 if the sample sizes were about 700. Such a study would be difficult and expensive to carry out, but is feasible.

The real issue here is whether such studies are worthwhile or whether we are willing to take the risk that operative mortality and recurrence rates may be somewhat higher in early discharge or outpatient surgery programs in order to reap their monetary benefits. What are these benefits? The studies reviewed here that did attempt to measure the benefits associated with these programs concluded that a small net benefit is present. It is difficult to assess how these studies might have been different in this regard had they been performed in the United States. Higher hospital costs might have increased the value of the net benefit, but higher wages might decrease it, given slightly longer convalescent times for short-stay patients.

Posing the question of whether the benefits are worth the costs raises difficult issues of how to trade off monetary savings for quality of medical care. In this instance, it certainly seems from the RCTs just reviewed that one trades a somewhat higher rate of minor complications for the monetary savings. And based on the statistical discussion just concluded, one may also be trading an unknown increase in the small

Figure B-1.—Regional Trends in Length of Stay for Patients With Inguinal Hernia



SOURCE: *Vital and Health Statistics*, series 13, Nos. 2, 10, 14, 17, 19, 23 26, 31, 41, 46, 55, 60, 64 (Washington, D.C.: National Center for Health Statistics, 1967-82).

but real risk of operative mortality or recurrence. Is the increased minor complication rate an acceptable side effect of these programs? Precisely what kinds of complications do these studies label as minor? The list is long. Morris (121) described chest and wound infections, hematomas of the wound, scrotum, and spermatic cord, unexplained fever, and thrombophlebitis. Ruckley (151) observed, in addition, a 23-percent rate of delayed wound healing in the outpatient surgery group as opposed to 14 percent in the hospitalized group. This usually consisted of a serous discharge from the wound. It is not clear from any of the reports how long these complications persisted or how much disability they caused. Presumably, all were short-lived. At best they seem minor annoyances, at worst potentially serious and debilitating illnesses.

How does one measure the cost of an additional 8 to 15 percent incidence of complications of this kind? Even were such a measure available, one would not have a reliable figure for gross savings calculated from a study performed in the United States. Does this lack of data and the possibility of increased operative mortality or recurrence in programs of early discharge or outpatient surgery justify further large and expensive RCTs? The studies reviewed here cannot provide answers to these questions but have provided the data necessary to formulate them.

Appendix C.—Health Program Advisory Committee and Acknowledgments

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*Until April 1983.

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